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THE PSYCHOLOGICAL REVIEW

IDEO-MOTOR ACTION

BY EDWARD L. THORNDIKE

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The theory of ideo-motor action has been for a generation one of the stock 'laws' of orthodox psychology. It is taught as almost axiomatic in standard treatises—is made the explanatory principle for phenomena of suggestion, hypnotism, obsessions and the like—and is used as the basis for recommended practices in education, psychiatry, religion—even in salesmanship and advertising.

In spite of contrary evidence brought forward by Kirkpatrick, Woodworth, Burnett, and others, probably nine out of ten of the members of this association believe, or think that they believe, in one or another form of this doctrine that an idea tends to produce the act which it represents or resembles or is 'an idea of,' or 'has as its object.'

Against this most respectable doctrine I early rebelled, and I somewhat greedily seize this occasion, which requires that in courtesy you listen to me for an hour, to justify this apostacy and convert you also, if I can, to the view that the idea of a movement, or of any other response whatsoever, is, in and of itself, utterly impotent to produce it.

The course of the argument will be plainer if I state first what potency I do attach to ideas of movements, or of the resident and remote sensations produced by movements, or of other results of movements; or to any image or other inner state of awareness which represents, or means, or is like, or has as its object, a movement or act or, for that matter, anything else. Any such mental state has, in my opinion, no

dynamic potency save that its physiological parallel will evoke whatever response is bound to it or to some part of it by inherited connections, or by the law of habit—including in the later the power of satisfying states of affairs to strengthen whatever connections they accompany or closely follow. I admit a slight tendency for a mental state which is produced immediately before and along with a movement—in one pulse of cerebral activity, as it were—to reinstate the movement by reinstating that total pulse of activity. The connections formed by the laws of habit work mainly forward, but slightly sidewise and even, indirectly, backward. The gist of my contention is that any idea or other situation tends to produce the response which heredity has connected with it or which has gone with it or some part of it with a satisfying or indifferent resulting state of affairs. An idea has no power to produce an act save the power of physiological connections born in man, or bred in him as the consequence of use, disuse, satisfaction and discomfort.

The doctrine of ideo-motor action, however stated, means that certain ideas or images have some further power than this—that between them and the responses which they represent, or have as their objects, or are 'ideas of,' or are similar to, some effective bond creates itself beyond what the connections made in the person's past can explain. Its classic statement by James reads, as you all know, "We may then lay it down for certain that every representation of a movement awakens in some degree the actual movement which is its object; and awakens it in maximum degree whenever it is not kept from so doing by an antagonistic representation present simultaneously to the mind."

Wundt asserts that the mere apperception of an image of a movement is followed by the movement unless some contrary force acts, and that in children and primitive men the presence of a vivid idea of a movement of their own bodies does therefore cause the movement to take place.

William McDougall writes to the same effect that: "In the special case in which the object to which we direct our attention by a volitional effort is a bodily movement, the movement

follows immediately upon the idea, in virtue of that mysterious connection between them of which we know almost nothing beyond the fact that it obtains," and elsewhere, "the representation of a movement of one's own body . . . like all motor representations tends to realize itself immediately in movement."

Two intelligible meanings can be attached to the phrases—'the representation of a certain movement by an idea,' an idea having a certain movement 'as its object,' an idea being 'of a certain movement,' 'motor representation,' and the like. The first is that the idea in question *is like* the movement—is to some extent a copy or correspondent of it in much the same way that the mental image of a square inch of red is like a square inch of red. The second is that the idea *means* the movement in much the same way that the thought of the words 'square inch of red' means such a square. For the sake of clearness I shall in general restrict argument to the first of these meanings of the doctrine, it being an easy task to disprove it in the second sense once it has been disproved in the first.

That the kind of an idea which is supposed by the ideo-motor theory to be able by some 'mysterious connection' to produce a movement is the idea which *is like* the movement appears more clearly in Miss Washburn's statement: "A movement idea is the revival, through central excitation, of the sensations, visual, tactile, kinesthetic, originally produced by the performance of the movement itself. And when such an idea is attended to, when, in popular language, we think hard enough of how the movement would 'feel' and look if it were performed, then, so close is the connection between sensory and motor processes, the movement is instituted afresh. This is the familiar doctrine expounded by James."

Professor Calkins still more explicitly states that in voluntary action we arouse a certain response by getting in mind an idea that is *like* the response.

An 'outer' volition being a volition to act in a certain way and an 'inner' volition being a volition to think in a certain way, "the volition is the image of an action or of a result of action which is normally *similar* . . . to this same action or

result. My volition to sign a letter is either an image of my hand moving a pen or an image of my signature written, and my volition to purchase something is an image of myself in the act of handing out money or an image of my completed purchase—golf stick or Barbédienne bronze." "Inner volitions," she adds, "do not so closely resemble their results. The volitional image of an act may be, in detail, like the act as performed"; but the volitional image of a thought is followed by only a "partially similar" thought.

The issue is now clear. Does an idea tend to produce only the movements which it or some element of it *has* produced (or accompanied in one total response), or does it tend also to produce the movement by which the sensory stuff of which it is the image *was* produced, and which it resembles?

I shall try to prove that an idea of a movement has, apart from connections made by use and satisfying results, no stronger tendency to produce the movement which it resembles, than to produce any other movement whatsoever,—no stronger tendency to produce what it represents or has as its object than an idea of an event outside man's body has—that, apart from connections made by use and satisfying results, the idea of throwing a spear or of pinching one's ear, or of saying 'yes' tends to produce the act in question no more than the idea of a ten-dollar bill or of an earthquake tends to produce that object or event.

Why should it? Why should the likeness between John Smith's mental image and some event in nature have any greater potency when that event is in the muscles of John Smith than when it is in the sky above or the earth beneath him? Why should McDougall's 'mysterious connection' be allowed to 'obtain' just here and not elsewhere?

The reasons why it should not are an attractive theme, but the evidence that it *does* not is our present concern.

First of all, an idea or image certainly *need* not arouse the movement which it represents, or 'is of.' Let each one of you now summon the most lively and faithful representation that he can of sneezing, then, after five seconds, of hiccoughing. Free your mind of any contradictory ideas, giving yourselves

wholeheartedly to thinking hard of the 'visual, tactile and kinesthetic sensations of sneezing.' We hear no universal chorus of nasal outburst or diaphragmic spasm. Either ninety-nine out of a hundred of you cannot get such representations of these movements as the theory requires or the theory is at fault. But if the theory requires a representation which not one person in a hundred can get of so definite and frequent and interesting a movement as a sneeze, the theory seems very dubious. As a matter of fact a large percentage of you would report that you could get as vivid and faithful an image of a sneeze as of the movements of your hand in signing your name or in handing out money.

To retort that sneezing and hiccoughing are not subject to voluntary control is futile. By the ideo-motor theory they *should be*. The retort witnesses rather to the fact that for a movement to be subject to voluntary control means not 'to be capable of representation in thought,' but 'to be connected as response by the laws of habit to some situation which one can summon at will.'

In the second place, in at least a majority of the cases quoted to support the ideo-motor theory—cases where an idea of a movement does have the movement as its sequent,—the connection can be shown to have been built up by habit—by use and satisfying results. When one has the idea of going to bed and goes, or of writing the word 'cat' and writes it, the explanation is found in the previous training that has connected the idea of going to bed with situations, such as being sleepy, to which the act is the original or accustomed sequent, or has otherwise connected the act of going to bed as response to the situation of thinking of so doing. The stock case most often quoted from James is that of a man getting out of bed—"The idea flashes across me, 'Hollo! I must lie here no longer'—an idea which at that lucky instant awakens no contradicting or paralyzing suggestions, and consequently produces immediately its appropriate motor effects." Here the idea is patently not a representation of the movement at all. The '*Hollo*' and '*I must*' show clearly that it is in words,[1] not in

[1] If by any sophistry it could be twisted into a representation of leg and trunk movements, it would be only the representation of lying still plus the idea of negation.

images of leg, trunk and arm movements. Its motor effects are appropriate, not in the sense of being in the least like it or represented by it, but in the sense of being the effects which that idea, when uncontested, had, by exercise and effect, come to produce in that man. The 'Hollo! I must' is a lineal descendant of the sensory admonitions from others received during life and connected each with its response by use, satisfaction, and the discomforting punishment attached to opposite courses.

In the third place, the supposedly crucial cases in favor of the ideo-motor theory really show the person *making the movement in order to get the idea of it.* Some of you have doubtless instructed your students as follows: "Try to feel as if you were crooking your finger, whilst keeping it straight. In a minute it will fairly tingle with the imaginary change of position; yet it will not sensibly move because *its not really moving* is also a part of what you have in mind. Drop *this* idea, think of the movement purely and simply, with all brakes off; and, presto! it takes place with no effort at all" (James's 'Principles,' II., p. 527). Now the essential fact here is that when anybody is told to try to feel as if he were crooking his finger, he tends, in the case of many subjects, to respond by taking an obvious way to get that feeling, namely, by actually crooking his finger. He responds to the request, regardless of any ideas beyond his understanding of the words, by a strong readiness to crook his finger. Being forbidden, he restrains the impulse. The 'tingling' is not from the *imaginary change* of the finger's position but from the *real restraint from* changing its position. The tingling occurs with individuals who cannot image the finger's movement. Far from showing that the imagined movement is adequate in and of itself to cause the movement, such cases show that it is unsafe to infer that the image comes first in cases where deliberately evoked images of movements are accompanied by the movements or parts thereof. If, in the experiment with ideas of sneezing, a stray individual does sneeze, it is ten to one that he has the rare power to make himself sneeze and has done so, intentionally or unintentionally, in order to get a more adequate idea of how it feels to sneeze.

These facts have long seemed to me adequate evidence that an idea can produce only what it, in whole or in part, has produced in the past, not what it is like or what it means. And I venture to hope that, by realizing just what the somewhat cryptic terms—to represent, to have as object, to be an idea of—mean and by noting just what happens in even the most favored cases for the production of a movement by an idea's likeness to it, you are made somewhat suspicious of the 'mysterious' and 'so close' bond by which every 'motor representation tends to realize itself immediately in movement.'

I shall now attack the doctrine from within, showing first that its own apostles think more highly of it the less clearly and emphatically it is stated, and even believe that the power of an idea's likeness to a movement to produce that movement is in inverse ratio to the amount of likeness—that the power of an idea to arouse the movement it is like grows greater, the less alike they are!

Last spring many of the members of this association kindly ranked in order of truth from four to ten statements concerning the general power of ideas to produce the acts which they resemble, or the power of some particular idea to produce some particular act. I take this occasion to thank them for their coöperation. These rankings, to which reference will be made repeatedly in what follows, represent a collection of judgments that are expert and, so far as my argument is concerned, impartial. Whatever errors of carelessness in reading, writing and the like affect them are such as have no prejudicial effect upon any of the conclusions which will be drawn from them.

From them we can measure the relative acceptability of each of a series ranging from clear and emphatic to obscure and mild statements of the power of motor representations to realize themselves in movement.

Consider, for example, these four statements:

30. A movement idea is the revival, through central excitation, of the sensations, visual, tactile, kinesthetic, originally produced by the performance of the movement itself. And when such an idea is attended to, when, in popular language, we think hard enough of how the movement would "feel" and look if it were performed, then, so close is the connection between sensory and motor processes, the movement is instituted afresh.

32. In the special case in which the object to which we direct our attention by a volitional effort is a bodily movement, the movement follows immediately upon the idea.

31. We may then lay it down for certain that every representation of a movement awakens in some degree the actual movement which is its object.

33. If a child or a primitive man has a vivid idea of a movement of his own body, that movement is thereby made unless it is prevented by some contrary idea.

The first two are obviously more emphatic statements of the doctrine of ideo-motor action than the last two, but they are less acceptable to a random picking from this association. Respect for the genius of James perhaps accounts for part of this, but other features of the returns show that the belief in ideo-motor action thrives on qualifications—turns gladly to 'a child' or a 'primitive man,' 'a vivid idea,' 'unless it is prevented,' and the like.

Consider next what should be the effect of attention to an idea upon the strength of its tendency to arouse the movement which it represents, supposing it to have such a tendency. Should we not, on all general principles, expect with Miss Washburn that 'when such an idea is attended to, when we think hard enough of how the movement would feel and look,' its power would be increased? Such seems the inevitable inference from consistent use of the ideo-motor theory. But, as will be seen still more clearly later, there is in the adherents to the theory a struggle between its principles and their sense of actual concrete facts; and the result here is that, in their concrete judgments, they deny the implication of the theory and insist that attention to the idea *weakens* its tendency to arouse the movement which it represents.

For example, the second of the two statements which I shall presently read differs from the first by supposing the movement-idea to be attended to (and also by supposing the idea to be one which resembles the movement a little more closely). The first statement is:

6. To make your spear fly straight and pierce the breast of your enemy it is useful to call to mind the sensations you had when, on other occasions, you saw your spear hurtling through the air straight at an enemy and striking him full in the breast.

The second is:

9. To make your spear fly straight and pierce the breast of your enemy, it is useful to think hard of the visual sensations, originally produced by the performance of the movement itself.

This association would vote over three to one that the second statement was the less true or more false.

The same point can be tested by two other statements from those rated. These are:

8. To make your spear fly straight and pierce the breast of your enemy, it is useful to imagine the sensations you had when, on other occasions, you felt the spear leave your hand, saw it fly through the air straight at an enemy and strike him full in the breast.

11. To make your spear fly straight and pierce the breast of your enemy, it is useful to think hard of the visual, tactile and kinesthetic sensations originally produced by the performance of the movement itself.

As before, the second statement adds the element of attentiveness (and also makes the idea in question a closer representative of the movement and emphasizes the kinesthetic element). This association would vote over two to one that the second statement was less true or more false than the first.

Still more damaging to the theory that ideas tend to evoke the movements which they resemble is the fact that, within certain limitations, the more closely they resemble them the less likely they are, according to your own judgments, to evoke them.

Among the forty statements rated were eight forming a series beginning with:

4. "To make your spear fly straight and pierce the breast of your enemy, it is useful to imagine the spear striking him full in the breast,"

in which, as you see, the idea is of a very remote result of the movement, not at all clearly like it or representative of it more than of many other movements. From this the series proceeded by graduated differences, through cases of closer and closer resemblance to the movement, to number 11, which was an almost verbatim adaptation of Miss Washburn's general statement to this particular case, namely:

11. To make your spear fly straight and pierce the breast of your enemy, it is useful to think hard of the visual, tactile and kinesthetic sensations originally produced by the performance of the movement itself.

The ratings show that although nine out of ten members of this association assert the truth of one or another form of the ideo-motor theory, their sagacious sense of fact compels them to go dead against it by assigning an order of truth to these

eight statements, directly opposite to that which the theory requires. You vote overwhelmingly that a mere picture of the spear striking the enemy is more likely to produce the proper cast of the spear than a full and exact representation of the movement itself. You vote that 'any idea tends to produce that act which it resembles' but you vote that the more it resembles it the less it tends to produce it! The first vote you cast under pressure from the 'steam-roller' of traditional orthodoxy; the second is the result of the 'direct primary' permitted by my questionnaire and reveals you as true progressives at heart.

If we let distance along a horizontal line *FT* stand for differences in truth, as judged by you, one foot equalling such a difference between two statements as seventy-five out of a hundred expert psychologists will distinguish correctly, No. 11, the statement concerning the close representative of the movement, is put nearly three feet *falser* than No. 4.

Some of you may suspect that my earlier phrase 'within certain limitations' conceals facts favorable to the ideo-motor theory. On the contrary, if time permitted, these limitations could be shown to be those expected by the habit-theory. The rule is that mere likeness does nothing; when, as here, an increase in likeness goes with a decrease in the strength of habit's bonds, likeness has the appearance of diminishing an idea's potency to arouse its act; when greater likeness of an idea to an act implies greater frequency of the idea as *situation leading to the act* in past behavior, then greater likeness has the appearance of increasing the tendency of the idea to arouse the act. Nor is the series quoted above a solitary or exceptional one. If one were free to get forty statements rated by each of you instead of four, one could report a dozen similar cases.

In general the ratings witness to a conflict in the minds of psychologists between adherence to the speculative doctrine that the conscious representation of a movement is, in and of itself, potent to produce it and a sense for concrete facts which insists that it is thus potent only when it has for some reason been in the past the situation leading to it. The theory claims

that an idea produces what is like it; observation teaches that an idea produces what has followed it.

Why, then, one naturally asks, did the theory ever gain credence, and why is it still cherished? The answers to these questions which I shall try to justify furnish my last and perhaps strongest reason why it should be cherished no longer. My answers are that the ideo-motor theory originated some fifty thousand years ago in the form of the primitive doctrine of imitative magic, and is still cherished because psychology is still, here and there, enthralled by cravings for magical teleological power in ideas beyond what the physiological mechanisms of instinct and habit allow.

Shocking as it may seem, it can be shown that the orthodox belief of modern psychologists, that an idea of a movement tends to produce the movement which is like it, is a true child of primitive man's belief that if you sprinkle water in a proper way your mimicry tends to produce rain, that if you first drag a friend into camp as if he were a dead deer you will be more successful in the day's hunt, or that if you make a wax image of your enemy and stab it he will tend to sicken and die.

Evidence that the accepted doctrine of ideo-motor action is homologous to, and a lineal descendant or vestigial trace of, the crassest forms of imitative magic may be sought along two lines—the comparative and the historical or, as the biologists would say, the palæontological. In comparative anatomy two forms of an individual or of an organ testify to a common ancestry—are rightly suspected of being homologous—in proportion as they are linked by intermediate forms and differ only, as we say, in degree. After a somewhat similar fashion I shall try to prove that the difference in falsity (or truth) between the absurdest magical superstititon and the most approved form of the ideo-motor theory is one of degree only, and that the latter is linked to the former by a chain of intermediate forms.

As magical superstitions we may take the following:

1. "To make your spear fly straight and pierce the breast of your enemy it is useful to make a wax image of your enemy with a spear stuck through his breast."

24. "If a man draws secretly a picture of you with the words 'Yes, I will!' coming out of your mouth and then asks you 'Will you give me your coat?' you are more

likely to answer 'Yes, I will!' than you would have been if he had not drawn the picture."

As what is in fact the most approved of the stock statements of the ideo-motor theory we may take James's familiar statement:

31. "We may then lay it down for certain that every representation of a movement awakens in some degree the actual movement which is its object."

Either of the two assertions of magical potency would be voted false by the association with practical unanimity. James's statement would be voted true by a comfortable majority. We regard the doctrine of imitative magic as sheer nonsense and the doctrine of ideo-motor action as substantially true. But our own judgments indicate that the latter is close kin to the former, when we treat them as we treat any set of judgments of difference in measuring the discriminability of objects.

Indeed only two intermediate links are required to show and measure the kinship. Recall Professor Washburn's statement:

30. "A movement idea is the revival, through central excitation, of the sensations, visual, tactile, kinesthetic, originally produced by the performance of the movement itself. And when such an idea is attended to, when, in popular language, we think hard enough of how the movement would 'feel' and look if it were performed, then, so close is the connection between sensory and motor processes, the movement is instituted afresh."

And consider also this vague statement, that:

5. "To make your spear fly straight and pierce the breast of your enemy, it is useful to imagine the spear hurtling through the air straight at him and striking him full in the breast."

These five statements, James's, Miss Washburn's, the one about an image of a hurtling and striking spear, the one about contemplating a wax image, and the one about writing in secret the words you wish a man to speak, differ, in respect to truth, only in degree. For people are able to compare them as to truth nearly or quite as readily and confidently as they can compare in respect to truth any five dubious statements chosen at random from psychological treatises. And when they so compare them the results are as follows:

Let the line *FT* represent a scale for truth. Let the point

marked 1. Magic represent the location on the scale of the truth (or falsity) of the statement (No. 1) about the potency of the wax image. Let each inch on the scale represent the amount of difference in truth necessary in order that seventy-five per cent. of this association shall judge the difference correctly, one out of every four being in error. Then statements 5, 30 and 31 are located as shown in Fig. 1. For the

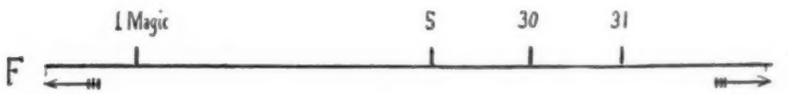


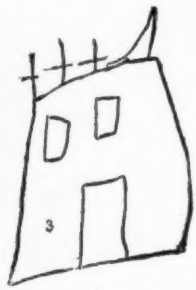
Fig. 1.

difference in truth between statement 1 and statement 5 is 1.64. It is measured by the fact that of 37 psychologists who compared them, 28 judged that 5 was truer or less false, 5 rated them as equally true or false, and 4 judged 1 to be truer than 5. The difference between statement 5 and statement 30 is .53. It is measured by the fact that of 24 psychologists who compared them 14 judged that 30 was truer or less false, 2 that they were equally true, and 8 that 5 was truer than 30. The difference between statement 30 and statement 31 is .5. It is measured by the fact that of 17 psychologists who compared them 9 judged that 31 was truer or less false, 3 that it was equally true and 5 that 30 was truer than 31.

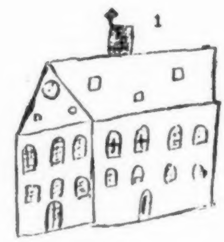
Thus the truth of statement No. 1—of the potency of fabricating a wax image of your enemy—is about $1\frac{2}{3}$ below the truth of statement No. 5—of the potency of vague thoughts about the spear striking him, $2\frac{1}{4}$ below the truth of Miss Washburn's statement and $2\frac{3}{4}$ below the truth of James's statement. The links are truly intermediate. The most approved statements of the ideo-motor theory are by their own advocates confessed to be only a little more truthful or less false than the rankest magical nonsense.

Using the same criterion of expert judgment in each case, the most approved statement of the ideo-motor theory is only about as far above a barefaced statement of the primitive superstition of imitative magic on a scale for truth as composition *B* is above composition *A* on a scale for merit in

English writing; or as drawing *B* is above drawing *A* on a scale for merit in drawing.



A.



B.

FIG. 2.

A

The boqk I refer to read is Ichabod Crane, it is an grate book and I like to rede it. Ichabod Crane was a man and a man wrote a book and it is called Ichabod Crane i like it because the man called it ichabod crane when I read it for it is such a great book.

B

First: De Quincys mother was a beautiful women and through her De Quincy inhereted much of his genius.

His running away from school enfluenced him much as he roamed through the woods, valleys and his mind became very meditative.

The greatest enfluence of De Quincy's life was the opium habit. If it was not for this habit it is doubtful whether we would now be reading his writings.

His companions during his college course and even before that time were great enfluences. The surroundings of De Quincy were enfluences. Not only De Quincy's habit of opium but other habits which were peculiar to his life.

His marriage to the woman which he did not especially care for.

The many well educated and noteworthy friends of De Quincy.

Some of you may be skeptical concerning this method of measuring differences of credibility in the minds of a given class of thinkers, harboring the suspicion that the individual reports were invalidated as measures of the individuals' opinions by the incommensurability of the statements. Some of you, indeed, refused to rate the statements. But, as a matter of fact, the main difficulty experienced with the various sets out of the forty-two statements which were issued was not that they were incommensurate as to truth, but that the differences were too small to be distinguished with any feeling of surety. I regret that the poll of this association is not complete, owing to the fact that some individuals refused, justly enough, to spend their time in grinding my axe, and that some canny ones

refused to be drawn into any testifying that might conceivably be held against them later. It seems, however, certain that the membership of this association experienced—or would have experienced, had they tried to make the comparisons—no greater sense of incommensurability than they would have experienced in grading advertisements for 'appeal,' drawings for skill, or poems for beauty. Reports of such difficulties were very rare.

Whatever validity attaches to your belief that you know what you are about and mean something real when you judge that James's familiar statement is less false than the assertion about the potency of secretly writing 'Yes I will' as a persuasive fetich, or that about the potency of wax constructions of one's enemy in warfare, attaches to all the comparisons that I have used. But had I asked for only this one comparison every member of the association would have made it with no sense of incommensurability or trickery, but only with a sad surprise that I should ask so foolish, because so easy, a question. This method of measuring differences in credibility in the minds of a defined group is in fact sound, and, I may add, is useful in the case of very many problems in the mental sciences.

In the present case it teaches us that our belief that an idea tends to produce the act which it is like, or represents, or 'is an idea of' or 'has as its object,' is kith and kin with out forebears' belief that dressing to look like a bear will give you his strength or that burning an effigy of the foe will make him die, and with the modern charlatan's belief that thinking one can walk will mend a broken bone. It is kith and kin with them, own grandchild of one and own brother to the other—and as false as either. "An image, idea or any other mental fact, has, apart from connections made by heredity, use and satisfying results, no stronger tendency to produce the movement which it resembles, or represents, or has as its object than to produce any other movement whatsoever—no stronger tendency to produce it than ideas of dollars and earthquakes have to produce them. Why should it? Why should the likeness between John Smith's mental image and an event in nature have any greater potency when that event is in the

muscles of John Smith than when it is in the sky above or the earth beneath him? Why should McDougall's 'mysterious connection' be allowed to 'obtain' just here and not elsewhere?" It obtains nowhere. The connection whereby likeness or representative quality, in and of itself, created a bond between a thought and an act, would indeed be 'mysterious' if it existed. But it does not exist.

THE ACCURACY OF LOCALIZATION OF TOUCH STIMULI ON DIFFERENT BODILY SEGMENTS[1]

BY SHEPHERD IVORY FRANZ

Although it has sometimes been recognized that the process of localization of a stimulus on the skin may be, or is, different from the discrimination of two or more stimuli, it has apparently been assumed that these forms of experience are intimately related to each other. The double point threshold and allied phenomena, such as the discrimination of lines, of filled and open spaces, etc., have been investigated by many, but there have been few investigations of the localization of single stimuli. That the two problems have relations with the general one of tactual space perception is apparently conceded, but that they are not alike and are not in direct relation with each other is not as well understood. Henri has noted that the error of localization is less than the double point threshold, and von Frey states that there is no direct relation, but the facts upon which these statements are based are not given. In certain clinical work a direct relationship has been presumed. That this form of relation does not exist will be demonstrated below. It has also been shown recently by the investigations of Ponzo[2] who determined the thresholds of localization of pain and touch stimuli in different parts of the body.

In the work reported by Ponzo, two subjects were used, the author and another skilled observer. On these subjects 25 regions were selected for examination and in each location 10 points were tested, 5 times each. The points selected for examination

[1] From the psychological laboratory of the Government Hospital for the Insane, Washington, D. C.

[2] M. Ponzo, 'Recherches sur la localisation des sensations tactile et des sensations dolorifiques,' *Arch. ital. de biol.*, 1911, Vol. 55, 1–14. In that article the author gives a resumé of his investigations, which were originally published, in 'Memoire della Reale Accademia della Scienza di Torino,' sec. 2, Vols. 60 and 61. At the present writing the originals have not been seen by me, and results of the work which are given in various parts of this article were taken from the above mentioned resumé by the author.

of touch localization were those sensitive to light touch stimuli, and the von Frey hairs, with values approximating the threshold in the different regions, were used as the stimuli. In any one region only 2 to 5 points were examined on one day. The selected points were marked with ink. The hairs were cut from regions where they were long. Before a stimulus was given the experimenter arranged the hand of the subject near the point to be stimulated in order that the movement of localization and the interval of time might be small. In this way the subject acquired definite information regarding the region to be stimulated, although the exact point was unknown. Presumably the eyes of the subject were covered both during the stimulation and during the localization. The localization was made with the tip of a hardened brush which was used by the subject to touch here and there until he was satisfied with his localization.

Ponzo found (1) that the average error of localization is always less than the corresponding double point threshold, (2) that this error does not appear to have any relation with the threshold of stimulation, (3) that the error of localization does not bear any direct relation to the double point threshold, and (4) that the average errors for the localization of pain and touch stimuli do not coincide, that for touch being usually greater than that for pain. The relation of Ponzo's results to the present work will be dealt with in subsequent sections.

In some examinations of skin sensibility in patients suffering from nervous diseases, I have noted that although the threshold for light touch appeared to be higher than the normal, the localizations of the touches were approximately within an area corresponding to the double point threshold. From the other data it appeared unlikely that the localization errors could be normal. An attempted comparison of these results with similar results in normal people failed, since no definite experiments regarding localization were found. It was because of this that the present series of tests were begun, and the experiments, with the exception of those on one subject, were finished before the resumé of Ponzo's results was brought to my attention.

Methods.—The methods used in the present work differed from those of Ponzo in many respects, although in general there are marked similarities in the results. The present work was undertaken largely for purposes of comparison with the abnormal. For this reason, separate touch points were not selected for stimulation, because the determination of these points in certain neurological or psychiatric cases would require

too much time, and in other cases it would be impossible. It was deemed sufficient to stimulate the skin by means of the pressure of a camel brush hair, which could be felt in all parts of the body, the pressure value of which approached the threshold, the brush used for this purpose being lightly bent at 100 milligrams and more strongly at 200 milligrams.

A stencil was made with small holes in parallel and vertical lines at intervals of 10 cm. This was placed upon a part of the body; certain landmarks, such as the mid-line, the umbilicus, the spinous process, etc., were located and marks were made on the skin with a grease pencil at each of the holes in the stencil. After the stencil had been removed other marks were placed between each two of the original marks so that these were to be found on the part at distance of 5 cm. Some of these points were selected for stimulation the extra marks being to prevent knowledge of the definite points which might be seen by the subjects during the course of the tests.

The stimulus was applied to one of the selected points, and the subject was instructed to locate the point by touching the skin as close as possible to the stimulated point. In the preliminary experiments, the subject was provided with a pencil for this purpose but in two cases it was suggested that the localizations might be more accurately made with the finger. Eventually the latter method was selected for and used by all subjects and the results which are reported were obtained by this method of finger localization. One subject reported distinct disturbance from the use of the pencil, because the localizing stimulus differed greatly from that of the brush, but it would appear to the writer that in this respect the use of the pencil would not be much different from the use of the finger. Another element may, however, be present, viz., that of familiarity with the localizing medium; and it is the writer's experience that localization can be made by him better with the finger than with a pencil or brush. Practice might change this result.

During the preliminary experiments the subjects were permitted to open the eyes for the purpose of localizing the stimulated points, but during these trials two subjects voluntarily gave up this method. During the progress of the regular tests the eyes were closed during the stimulations and localizations, and the subjects were not made acquainted with the accuracy of their localizations or the degree of the variations.

After a point had been stimulated the subject localized the stimulated point in the manner described above. When the finger was placed upon the skin, it covered an area of about 1 cm. in diameter, and within this area it was impossible to determine the exact point which was supposed by the subject to be the stimulated point. To make a definite record, the approximate center of this area was taken as the localization of the subject. One subject who had a long nail on the forefinger, used it as the pointer, and in this case (Subject *E*) the localization was more accurately determined. A glance at the tables will show, however, that this subject was no more accurate in her localizations than any other subject who used the coarser method. The points which were touched by the subject, or the centers of the circles of the finger touches, were marked with another colored grease pencil, and these records became permanent on the skin for the time of the experiments on one day.

After a series of tests on one day, in which a certain number of points had been stimulated 10 or 15 times each, a piece of transfer paper, about 15 cm. square, was placed over a point, and on it were marked the stimulated point, the 10 or 15 localizations, and neighboring points to give the relations to bodily landmarks. In this way, vertical and horizontal ordinates could be drawn and constant tendencies determined. After these records had been made, the marks on the skin were wiped off with xylol.

The results which were recorded on transfer paper were then transferred to paper on which permanent horizontal and vertical ordinates were drawn, the stimulated point being placed at the center of these ordinates. The localizations were pricked through, and from this pricked record, readings were made both of the distance from the point stimulated and from the ordinates; the former for the determination of average errors and the latter for the determination of the average constant errors. A form made by drawing concentric circles, 5 mm. from one another, was made, and this was used for reading the final transfer record. It was counted that all localizations within the first circle averaged 2.5 mm. error, those within the second circle 7.5 mm. error, etc. There is, therefore, a possibility of error in these calculations of about 2.5 mm., but since the area of localization (the finger tip) was as much as 10 mm. in diameter, the error of calculation is well within this error. In one series, the pricked records were accurately measured to within the nearest half millimeter, and the averages were compared with the averages by the concentric circle method. There was such a slight difference between the results by both methods, especially when compared with the average variation, that subsequently the easier method was alone employed.

Sixty points were selected for testing. These were at least 10 cm., often farther, apart. Although the subjects differed in size the absolute distances of the points on any one part were kept constant, although on neighboring parts this relation might be slightly different for the individual subjects. Certain parts of the body were not tested on account of the impossibility

of touching these parts after they had been stimulated. Thus, for example, the shoulder blade areas and parts of back were not included, since the subjects could reach these parts only by making awkward and consequently disturbing movements. For this reason, on the back, only two points were selected for examination—one below the shoulder blade area and one at the level of the hips, both of these in the middle line so that they could be reached by either hand. On each anatomical part, *e. g.*, legs, thorax, abdomen, etc., the same number of points was not used on account of the desire to have the individual points at least 10 cm. apart.

The points on the back have already been described. The others are as follows: On the chest; one point was selected at the mid-line at the level of the nipples; 2 points on either side 5 cm. below but 10 cm. from the mid-line; above, one point on each side, 10 cm. from the mid-line and 10 cm. above the level of the first point; one point at the mid-line which came about at the junction of the neck, with the thorax; and points on either side of this and level with it, 15 cm. from the mid-line, making 8 points in all. On the abdomen; 2 points at the level of the umbilicus and 5 cm. on either side; on lines above and below at a distance of 10 cm. from this line, 3 points each, respectively at the middle, right and left sides (10 cm. from the mid-line), making 8 points in all. On the upper portion of the legs 5 points were selected; in front, 5 and 15 cm., respectively from the mid-line of the body, and on a level with the vulva; on a line 10 cm. below this, 2 points; one on the inner surface, the other directly in front about half way to the knee; and a point 10 cm. below these, about 5 cm. above the knee. On the lower portion of the leg three points were selected; half-way between the knee and the ankle, one in front, the other two on a line around the leg, each at a distance of 10 cm. from the front. There were two points on the face; on either side, on a line with and half way between the nose and the tip of the ear. On the outside of the arms when extended, 6 points were made; beginning at the shoulder, and continuing in a line with the middle finger at distances of 10 cm. On the inner aspect of the arm; the first point was about 10 cm. from the axilla and 4 others at 10 cm. distances, and in a line drawn to the middle finger. On the upper arm, therefore, there were 5 points and on the lower arm, 6. On the sole of the foot, one point was selected about the middle.

An attempt was made to have 25 localizations for each subject on each point, and these were divided into two series of 10 and 15 respectively. After the preliminary experiments the series with 10 stimuli was made on all of the points which were selected. After this series was completed the second series, in which 15 tests was the basis, was made, and the results in the two series are given in the accompanying tables. With each of two subjects only one series was made.

At some sessions only one section of the body, *e. g.*, the

right arm, was tested. At other sessions, areas quite separate were tested. Thus, for example, it was thought advisable to test in an irregular order the arms and thighs, or the legs and abdomen and chest, so that the subject would not have too much idea of the exact localization of the stimuli which were to be applied. In this way a check was made upon the results when only one section of the body was being stimulated. It was thought that knowledge of the part to be stimulated might decrease the errors but the comparison of localizations by these two methods of procedure showed that the localization when the stimuli were successively applied to different regions was no less exact than when one part was alone stimulated.

Subjects.—The subjects who are used in this work were all accustomed to observations and examinations of the different parts of the body. They were artists' models, women who had been engaged in that capacity from one to ten years.[1] The importance of this will be appreciated by those who have had occasion at any time to make examinations or tests on both normal and abnormal subjects. Their attitude toward the tests was, as far as could be determined, absolutely indifferent except in the preliminary tests which were not recorded. Conversation with these subjects showed that there was some nervousness on their part in coming to take part in the tests, but this was explained by all as having been due not to the exposure of parts of the body, to which condition they had become accustomed, but rather to an ignorance of what would be required. As soon as the subjects became acquainted with the object of the experiments, and this was rather carefully explained to them at the first session, the nervousness disappeared, and their relation to the experiments became,

[1] The selection of women subjects was not made because they could be obtained more easily or were considered to be better observers than men, but because it was believed that the presence of long bodily hair on man might introduce a factor into the localization. In the work of Ponzo on the areas selected for examination, the hairs were cut. In comparing the localization errors on parts well endowed with, and on others with little hair, I found no correlation between the length of the bodily hair and the localization error. In most of the subjects, I measured as well as was possible in a casual examination, the average length of hairs on the different parts which were stimulated and the relation of this factor to localization is discussed in a later section of the article.

so they reported, a perfectly normal one. Part of the nervousness which the subjects exhibited at the beginning was also due to being asked to come to a hospital for the insane. It is probable that unfamiliarity with conditions in such institutions and the general feeling about them were large factors in the mental disturbance. None of the subjects were troubled in this respect after the first day.

The education of these subjects was fairly good and all were intelligent observers, although most of them had had no previous experiences as subjects in psychological or other scientific work, previous to the present series of tests. One, however, had been used in expriments of a like nature by me two years previously, and knew, therefore, something of the general character of the work. The introspections, or observations of the conditions of the tests and of their own sensations, were, as might be expected, quite naïve, but were, I judge, as good as those which are to be obtained from students in an introductory course in psychology, and of about the same character. The ages of the subjects varied from 23 to 35 years. The following data regarding the individual subjects are given because of a possible relation these facts may have to individual peculiarities of sensibility. At the present time correlation of these facts with other individual peculiarities is not possible. Subject *A:* Married, several children; nothing abnormal in history. Subject *B:* Widow, one child; has had seven years' experience as artist's model. Following the death of her husband eight years ago, she had what was called a neurasthenic attack, at which time there was a very marked hypoesthesia, amounting at times to an anesthesia and an analgesia; at the same time, she exhibited a certain lack of coördination in her movements, and for about a year, this was evident in performing such habitual acts as combing her hair, etc.; the hypoesthetic condition gradually disappeared, but the last to become well was the left lower arm. Subject *C:* Sister of subject *B;* unmarried; nothing abnormal in her history was ascertained. Subject *D:* Married; several children; no history of abnormalities was obtained. Subject *E:* Married; no children; artist's model for about five years. Subject *F:* Not an artists' model; used in only a few tests which were incidental to other examination. Consulted me regarding a feeling of numbness in the left hand along the area of ulnar distribution. Besides a general neurological examination, I took the opportunity of making a few tests on the sensibility of parts which might be compared with the supposedly hypoesthetic area. The experiments in this case were not a complete series, only a few points on the chest, abdomen and each arm being taken for purposes of comparison. The results of the examination of the supposedly abnormal areas are not used in the present paper. It may be mentioned, however, that nothing abnormal was discovered; probably the hypoesthesia was a paresthesia.

The subject completely undressed in a warm room, and was then directed to lie on a couch with a covering, usually a sheet and blanket, to keep the body warm and protected from drafts, etc. At the time of the experiments, the part of the body which was being tested was uncovered, and along with it usually the right arm. At times larger areas of the body were uncovered, and at times owing to the nature of the

experiments, no covering was used. For example, when the left arm was being tested, the remainder of the body was covered. When the chest was being examined, it and both arms were uncovered. When the abdomen was being tested, the whole upper half of the body was uncovered; and when the back was tested, the patient sat on the couch with her back to the experimenter, and the whole body was uncovered.

Results

Average Errors of Localization.—In the calculation of the results the individual errors for all points on a segment of the body were grouped, so that in the upper arms there were 10 points; in the lower arms, 12 points, etc. A comparison of the results from the individual points in the larger segments showed a marked uniformity, although there were individual variations, and the grouping was believed to be sufficiently accurate. The average errors of localization by the six subjects who were examined are given in the accompanying tables. Two series of 10 and 15 tests on each point were made with subjects *A*, *B* and *C*. In the tables these are given separately. The figures in the tables give first, the average error; next, the average variation of the error; and, finally, the total number of experiments on the particular segment. In this table no division is made between the tests on the right and those on the left side of the body.

In the comparison of the results in Table I., it will be noted that the average error of localization for all the subjects is approximately the same on the same parts of the body. There are individual differences, which are comparatively slight in amount, but, on the other hand, there are some noticeable individual differences in the relative sensitivity (measured by the localization error) of the parts. Thus, for example, the most accurate localizations by *A* are on the cheek, abdomen and foot, the least on the back. On *B*, the cheek is the most sensitive; the forearm, chest, abdomen and lower part of the leg are approximately the same; and the least accurate localization is on the upper part of the leg. On *C*, the most accurate localization is on the cheek, the least on the back. On *D*, the

Table I

Figures in large type give average errors of localization in mm.; smaller figures, average variations; those in parentheses the number of tests for each average. For numbers of points stimulated, see text.

Subjects. Parts of Body.	A		B		C		D	E	F
	Series 1	Series 2	Series 1	Series 2	Series 1	Series 2			
Cheek......	4.5 2.4 (20)	6.0 2.8 (30)	4.8 2.5 (20)	4.5 2.4 (30)	6.8 3.0 (20)	5.8 2.9 (30)	5.0 2.5 (20)	7.2 3.7 (30)	—
Lower arm ..	16.9 8.8 (120)	16.0 8.9 (180)	15.1 7.0 (120)	12.8 5.5 (180)	16.9 10.1 (120)	13.7 7.1 (180)	21.5 11.9 (120)	23.1 10.8 (180)	22.8 10.7 (15)
Upper arm ..	15.7 7.2 (100)	16.4 8.0 (150)	17.2 8.2 (100)	15.9 7.4 (150)	13.4 6.6 (100)	17.9 8.8 (150)	21.2 9.6 (100)	28.5 13.4 (150)	—
Chest.......	17.2 7.9 (60)	13.8 6.2 (120)	14.2 6.9 (80)	15.0 6.1 (120)	16.1 6.7 (80)	19.4 9.0 (120)	18.8 8.8 (80)	19.6 7.5 (145)	14.9 6.8 (20)
Abdomen ...	16.7 6.0 (80)	13.9 6.0 (120)	15.6 6.2 (80)	14.8 6.9 (120)	20.0 9.3 (80)	14.4 6.7 (120)	20.8 11.7 (80)	24.0 12.4 (135)	20.5 10.0 (15)
Back.......	32.3 11.3 (20)	27.5 8.3 (30)	19.0 10.3 (20)	16.7 6.3 (30)	26.5 7.2 (20)	21.3 9.7 (30)	27.0 14.0 (20)	12.8 6.4 (15)	—
Upper leg ...	17.5 8.4 (100)	17.9 8.0 (150)	18.7 8.0 (100)	18.0 7.7 (150)	15.7 6.6 (100)	19.6 9.6 (150)	20.7 8.8 (100)	20.8 8.8 (150)	—
Lower leg ...	23.2 8.5 (60)	14.1 6.3 (90)	13.8 7.2 (60)	16.7 6.2 (90)	17.8 9.4 (60)	16.9 8.0 (90)	15.7 7.9 (60)	33.6 16.2 (90)	—
Foot.......	15.5 4.3 (20)	12.5 5.3 (30)	8.0 3.3 (20)	11.2 4.0 (30)	15.0 7.8 (20)	9.1 4.0 (30)	8.0 3.2 (20)	15.5 7.5 (30)	—

Table II

General Averages for 5 Subjects

Figures in large type give average errors of localization in mm.; smaller figures, average variations of the individual averages; those in parentheses, the total numbers of tests.

Parts of Body

Cheek	Lower Arm	Upper Arm	Chest	Abdomen	Back	Upper Leg	Lower Leg	Foot
5.7	17.9	19.8	17.2	18.2	22.1	19.1	19.9	11.7
0.8	3.5	4.0	2.0	3.4	5.5	1.3	5.5	2.3
(200)	(1,200)	(1,000)	(805)	(815)	(185)	(1,000)	(600)	(200)

greatest accuracy is also found on the cheek, then the foot, and finally the back; and on *E*, the cheek is the most accurate and the lower portion of the leg is the least.

It will also be noted that the average variations are comparatively large. This sometimes amounts to as much as, at times more than, 50 per cent. of the total, although occasionally the variation is as little as 25 per cent. A similar relation between the variations and the average was found by me in some earlier experiments on the threshold of touch on different parts of the body, and it seems likely that this apparently great variation is not remarkable but is inherent in the nature of the sensory mechanism.

In the next table, there are given the averages of the averages, for all subjects, excepting *F*, which show that the cheek is the most sensitive, and, in order, the foot, the chest, forearm, abdomen, thigh, the upper arm, the lower portion of the leg, and the back. The small amount of variation in these averages is worthy of notice.

In the present series of tests, the variations observed between the values on individual segments, noted in Ponzo's results, were not found. Thus, Ponzo notes the average localization values (in mm.) for his two subjects on 4 points on the chest, as follows: 17.05, 8.62, 6.65, 6.50; on 3 points on the back: 13.03, 13.72, 7.65; the average variations of Ponzo's results are as great as those noted in the above tables, so that the constancy of localization is apparently no greater than that found in the present series. Grouping the averages of Ponzo's results into segments, it is found that the localization accuracy is greatest on the face, and, in order, come the foot, abdomen, forearm, chest, back, leg, upper arm, thigh. The only marked variation between these results and mine is in the relative positions of the thigh and back, and these positions might easily have been reversed if more subjects had been tested.

Localization of Light and Heavy Touches.—During the course of the tests, some of the subjects reported that it was difficult for them to localize the light touches which were given with the brush. Since this stimulus might not always be useful for clinical examinations, on two subjects I made observations of the relation of light touches and more marked pressures, given with the brush handle, to the average error of localization. The results of these tests are given in the accompanying table. It will be noted that the heavier pressures did not increase the accuracy of localization as compared with those of light touch. With *C*, the experiments were made

on the back so that she might not be able to see the parts which were stimulated. Two series of experiments were made on two points, 15 with light and 15 with heavy touches on each. The average localization threshold in the touch test was only 16 mm. (av. var., 7.2), and with pressure, 30.3 mm. (av. var., 11.0). *A* was tested in the same manner in two series on the abdomen and thigh, two points being selected each time, and the point selected at the first examination for the stimulation of touch, being selected as the point of stimulation of pressure at the second examination. The results show: on the abdomen the same average error of localization (9.0 mm.), but the average variation for the pressure experiments slightly exceeding that for touch experiments (4.8 and 4.6, respectively); on the thigh, the average error of localization of the pressure stimuli was greater than that for the touch (17.0 mm. and 14.8 mm., respectively), and the average variations in these tests were correspondingly large (6.0 and 3.5, respectively, for pressure and touch).

TABLE III

AVERAGE ERRORS OF LOCALIZATION FOR LIGHT TOUCH AND PRESSURE

Figures in large type give average errors of localization in mm.; smaller figures, average variations; figures in parentheses, the numbers of tests.

Subjects	Parts of Body	Stimulus	
		Touch	Pressure
C	Back.	16.0 7.2 (30)	30.3 11.0 (30)
A	Abdomen.	9.0 4.6 (20)	9.0 4.8 (20)
	Upper leg.	14.8 3.5 (20)	17.0 6.0 (20)

In corresponding tests by Ponzo, with more carefully measured stimuli, a somewhat similar effect was found, although the author concludes the error of localization is inversely related to the pressure. Ponzo used stimulus hairs of 1.0, 1.5, 3.0, and 10 gr. mm. force, and found in these tests the error tended to increase as the pressure increased. The average variations in these tests are so large that this definite conclusion appears unjustified, although there is an indication of this relation both in the work of Ponzo and in that of the present series. For purposes of comparison, Table IV. is appended to give Ponzo's results. It should also be noted that Ponzo found less error

in localization from punctiform pain stimuli than from the punctiform touch stimuli; in only two locations were the averages for these forms of stimulation approximately the same, viz., the forearm, and the point of the tongue.[1]

TABLE IV

LOCALIZATION ERROR IN RELATION TO STRENGTH OF STIMULUS (PONZO'S RESULTS)
Smaller decimals omitted

Stimulus	Average Errors	Average Variations
10 gr. mm.	10.7	3.8
3 gr. mm.	10.6	3.9
1.5 gr. mm.	10.7	4.6
1 gr. mm.	9.8	4.1

Constant Errors.—In the description of methods, it was noted that the localizations were recorded in such a manner that the constant errors could be determined. For two subjects these were calculated, but since these appeared to vary considerably in different parts and there seemed to be no definite constant error, they were not calculated for all subjects. The average constant errors for these two subjects are given in the accompanying table. It will be observed that for certain parts there is harmony, while for other parts there is considerable difference. Thus in the second series, with *A*, on the forearm, in one series there was a tendency to locate the stimulus slightly towards the ulnar border, while in the other series, there was a slight, perhaps negligible, tendency to locate the stimulus radial-wards. On the upper part of the leg, similar variations were noticed. If the figures are to be considered to have any value at all, there is an opposition, which make the results doubtful, and there can not be said to be a constant tendency.

The examination of the constant errors in the investigation of Ponzo, leads to the same general conclusion. The variations are so marked for the two subjects, for the different segments and for the individual points, that no general conclusion can be

[1] The relation of threshold value for touch and localization error was also investigated by Ponzo, who found that stimuli on points which have the same threshold value are not equally well localized. Thus, he found points on the wrist, forearm, neck, upper arm, thigh and foot with the same threshold value, but the average error of localization in these places varied from 4.6 mm. (foot) to 17.4 mm. (thigh). A comparison of the localization values obtained by me with those on the touch threshold (not on the same subjects) show a greater correspondence than those of Ponzo (see Franz, 'Touch Sensations in Different Bodily Segments,' Government Hospital for the Insane Bulletin, No. 2, 1910, 60–72).

reached, except that there is no general tendency to locate stimuli in any particular direction.

Table V

Average Constant Errors of Localizations, in Mm.
Figures in parentheses are numbers of tests

Subjects	*A*		*C*
Parts of Body	Series 1	Series 2	Series 1
Cheek	Towards nose, 1.8 Upwards, 2.8 (20)	Towards nose, 1.8 Upwards, 3.8 (30)	Towards nose, 2.7 Upwards, 2.5 (20)
Lower arm	Ulnarwards, 2.3 Downwards, 11.3 (120)	Radialwards, 0.8 Downwards, 4.5 (180)	Ulnarwards, 0.7 Downwards, 3.1 (120)
Upper arm	Ulnarwards, 6.5 Upwards, 0.6 (100)	Ulnarwards, 0.4 Upwards, 6.0 (150)	Radialwards, 0.9 Downwards, 6.2 (100)
Chest	To left, 4.2 Downwards, 4.9 (60)	To left, 0.9 Downwards, 1.2 (120)	To right, 6.0 Upwards, 1.5 (80)
Abdomen	To right, 2.3 Downwards, 12.2 (80)	To right, 1.1 Downwards, 7.9 (120)	To left, 2.3 Upwards, 0.2 (80)
Back	To left, 0.9 Upwards 31.3 (20)	To left, 0.7 Upwards, 27.6 (30)	To left, 7.7 Upwards, 16.7 (20)
Upper leg	Outwards, 0.1 Downwards, 7.6 (100)	Inwards, 0.6 Upwards, 1.0 (150)	Inwards, 3.6 Downwards, 2.4 (100)
Lower leg	Inwards, 0.6 Downwards, 19.5 (60)	Inwards, 2.1 Downwards, 9.3 (90)	Outwards, 2.2 Upwards, 7.0 (60)
Foot	To toes, 4.6 Outwards, 10.0 (20)	To toes, 12.0 Inwards, 0.5 (30)	To toes, 1.4 Outwards, 3.3 (20)

Although the results of the examination of the tests showed no conclusive evidences of any constant tendency to localize the stimuli in definite directions, it appeared that some tendency was present, and for this reason an additional set of observations were made in the cases of the subjects *A*, *B* and *D*. In these subjects, a stimulus was given at a point, let us say on the chest midway between the nipples, and the subject located this point immediately thereafter. After an

interval so that the effect of this first stimulus had passed away, the point which had been located and marked, was taken for the point of the second stimulation. When this point was located, the localization was taken as the point of stimulation and so forth. In this way a number of tests on different parts of the body was made.

With *A* the original point, midway between and on a level with the nipples, was stimulated and in 10 tests, the subject reached a point 8.5 cm. from the mid-line and 17 cm. below the first stimulation, making an average constant error of about 8.5 mm. towards the left and 17 mm. downwards. In a second experiment on a subsequent day in 10 tests the point was reached on the left 10 cm. from the mid-line and 16.5 cm. below the first stimulation. In a third series, a point was reached after 10 tests, 11 cm. from the mid-line, and 9 cm. below the first stimulus. In all of these tests on different days, there was a decided tendency to locate the stimulus towards the left and downwards. In another series, the first stimulus was applied in the axillary line, about 9 cm. to the left of the nipple, and in 10 tests the subject finally reached a point 16 cm. nearer the mid-line and 6 cm. below the nipple. Here we have a tendency to the location in the opposite direction from those which are first described, although there is a slight tendency to locate the stimulus downwards. In a series of six tests in which the first stimulus was given immediately above the areola of the right nipple, the final localization was at a point about 5 cm. to the left and 8 cm. below the first point stimulated. Similar tests with the other subjects gave similar results, both on the chest and on the thigh. In some of these experiments, there was a tendency to localize inwards and downwards, and in others, upwards and outwards. No general tendency in this respect was found, and two series on one subject did not always give corresponding results. From the results of these tests, the conclusion may also be drawn that no general constant tendency to localize in particular directions is present.

Comparison of Average Localization Error and Double Point Threshold.—The results which have already been recorded

indicate plainly that the localization thresholds are not as large as the double point thresholds, and from inspection it appears that these two thresholds bear no definite relation to each other. Since we have for comparison only general data regarding the double point threshold, it was deemed advisable to extend the work by making a brief series of tests of the double point threshold on some of the parts which were examined for the localization threshold.

At the same sessions that *A* was tested for the comparison of the thresholds for light touch and for pressure she was also tested for the double point threshold on the same parts. On the abdomen, the average localization error for light and heavy touch was 9 mm.; on the thigh, it was 14.8 mm. for light touch and 17 mm. for pressure. The double point threshold for the abdomen was tested in a series of experiments in which about each point 60 experiments were made with double stimuli, and 60 with single stimuli. When the points were 4 cm. apart, the double stimuli were appreciated 55 times (92 per cent.), and when the points were 3 cm. apart, 27 times (45 per cent.). On the thigh, only 30 (*i. e.*, 30 double and 30 single stimuli) experiments at 4 cm. and 5 cm. each were made. It was found that when the points were 4 cm. apart, there were 14 correct responses (47 per cent.) to the double stimuli, and at 5 cm., 23 (77 per cent.). Selecting as the double point threshold that distance which two points must be to be sensed as double 75 per cent. of the times, on the abdomen the threshold is about 3.5 cm. and on the thigh about 5 cm. The average localization error on the abdomen is only about one quarter that of the double point threshold, and the localization error on the thigh is about one third of the double point threshold. Apparently in this case, there is no constant relation between these two processes.

In *C*, similar experiments were made and comparisons drawn for the back. It will be remembered that in this subject the average localization error for pressure was about twice as great as that for light touch. On the same points on the back, the double point threshold was determined by stimulating with points at distances of 4 cm. and 5 cm. On the upper part

of the back with the points 4 cm. apart in 100 tests (100 single and 100 double stimuli) 78 double stimuli were sensed as double, and on the lower part of the back, in a similar series, only 73. At 5 cm., on the upper part of the back, 85 of the double stimuli were properly appreciated and 89 on the lower part of the back. The double point threshold in these cases may be considered to be about 4 cm. The average localization error in the same regions is only 16 mm., viz., about two fifths of the double point threshold.

On *E*, the experiments for double point threshold were made at different sessions so that a comparison of these results with those on the localization error can not be made under the same dates. The results may, however, be considered fairly characteristic in view of their similarity to those obtained with *A* and *C*. The number of tests on each part was not the same. In 110 tests (110 single and 110 double) 88 (80 per cent.) of the double stimuli on the chest were appreciated at 4 cm., and 93 (85 per cent.) at 5 cm. In tests on the abdomen, experiments at 4 cm. and 5 cm. were performed at different sessions and the results of these tests are not harmonious; at 4 cm., there were 68 (85 per cent.) correct in 80 double stimuli and at 5 cm., only 32 (80 per cent.), in 40 double stimuli. It is possible that the fewness of the experiments in the 5 cm. series does not give a proper value and they are to be considered only as confirmation of the results in the 4 cm. On the upper arm at 6 cm. in 48 experiments, 26 (55 per cent.) of the double stimuli were correctly appreciated, and on the lower arm in 40 tests at 6 cm., 33 (82.5 per cent.); on the thigh, in 80 tests at 4 cm., 55 (69 per cent.) were correct; on the lower leg at 6 cm., in 40 tests 32 (80 per cent.), and at 4 cm., 11 (27.5 per cent.), in 40 tests. With this subject, it was impossible to get additional time for further tests and the series is incomplete in many respects, but the figures are sufficiently indicative of the double point threshold to warrant consideration in connection with those which were made with subjects *A* and *C*. It may be said that the double point threshold on the chest is less than 4 cm., and it will be observed that the average localization threshold is about one half of this. On the abdomen in this

subject, the average localization error is more than one half of the double points threshold, which may be considered to be less than 4 cm. On the upper arm the double point threshold probably may be considered to be 7 cm., and the localization threshold is about two fifths of this; on the lower arm the double point threshold is about 5.5 cm., and the localization threshold is only one half as great. On the upper part of the leg, the localization threshold (20.8 mm.) is only about one half that of the double point threshold (45 mm.). On the lower leg, the double point threshold is about 55 mm. and the average localization threshold is 33.6 mm., about three fifths.

Length of Bodily Hair and Localization Threshold.—Although the bodily hairs have a complex nerve supply, part of which is probably sensory, the sensory phenomena accompanying the disturbance of the hairs have not been carefully investigated. That the stimulation of a hair produces a sensation different from that of light touch has been shown by the results of examination of patients with divided nerves. What part this form of sensation plays in localization is not known. It was believed that the localization of light touch might be correlated to a certain extent with the length of the bodily hair, but the facts fail to show a definite relation.

The longest bodily hairs (excluding the pubic, axillary and head regions) on *A* were on the lower part of the leg, 13.5 mm., double the length of those on the thigh, 6.5 mm., although the average localization thresholds in these parts are the same. The length of the hairs at the two points on the back was the same as that on the thigh, but the localization error on the former was 50 per cent. greater than that on the latter. Similarly with *C*, on the back the hairs measured 3 mm., and on the upper arm, 4 mm., but the localization error on the latter was only about 70 per cent. that on the back. On *B*, the length of the bodily hair was not measured, but the results of the tests on *D* and *E* show no more definite relation of hair length to localization error than those on *A* and *C*.

Practice Effects.—A comparison of the results on each subject which were obtained upon the first day of careful testing (the second session of the tests) with those which were obtained upon the final day failed to show any difference in accuracy of localization. It is true that these tests did not extend over long periods of time, the maximum time being about one month, so that perhaps we should not expect any considerable effect from this amount of training. On the other hand, however,

we should expect the greatest amount of education in this regard during the early days of the experiments. It is possible that the results of the preliminary tests on the first day were less accurate than those at subsequent sessions. If so, this was not determined, for in these experiments no records of the tests on the first day were made.

Dyschiria Effects.—It is well known that when the hand is placed upon a table, and one finger, let us say the medius, is stimulated close to its junction with the hand, at times the stimulus is located on that finger, and at times on the finger next to it, so that under certain circumstances a touch on the side of the medius finger next to the index finger is sometimes located as a touch on the latter, and a touch on the side of the medius next to the third finger is sometimes felt as a touch on the latter. A similar dyschiria phenomenon was found in the experiments which were performed in this series.

When, for example, the subject's arms were placed along the sides of the body, and the chest near the axilla was stimulated, the touch was sometimes believed to be due to the stimulation of the arm and was localized in the latter place. When the arm near the axilla was stimulated, the reverse phenomenon was observed, and the touch was sometimes localized on the chest. This phenomenon was never observed when the arm was stimulated below the elbow, and the number of times that the wrong location was given for the stimulus appeared to be almost in proportion to the distance of the stimulus from the axilla. In these tests the error of localization was not large, if the absolute locations of arm and chest be considered, but the amount of error was great when the arms were extended at right angles to the chest and the measurements made in this way. Measured as the arms were placed during the tests, at no time was the error more than 1.5 cm. Since in a few tests the localization was made a quarter of the distance between the axilla and the elbow, the total error, measured from the point stimulated on the chest to the axillary fold and thence to the located point on the arm, was very great, and in one case this amounted to 14 cm. In only two subjects was a stimulus located on the arm when it was given on the chest

farther than 3 cm. from the axillary fold, and, vice versa, on the chest when the arm was stimulated farther than 3 cm. from the axillary fold.

A similar result was found on the legs when the upper inner parts of the thighs were touched. Sometimes the touch was located on the other leg, and these contralateral localizations decreased in number as the stimuli approached the knee. Although the calves touch when the legs are extended parallel, in no case was a stimulus to this lower part of the leg localized on the other side.

In tests of this character we may have conditions similar to those in the first part of Aristotle's experiment, in which a pea or a shot or normally opposing parts of neighboring fingers is felt as a single stimulus. In this way the localization of touches on one finger when the opposing part of its neighbor is stimulated may be explained, and also the wrong localization of touches on arm or chest near the axilla; and those on the upper inner parts of the legs. Another suggested explanation for this mild form of dsychiria is that the touch, however light, may disturb not only the part which is stimulated, but also the neighboring parts. The later hypothesis was tested to some extent in regard to those parts in which the dsychiria effect was noticeable and those in which it was not observed. In a number of experiments special care was taken that the light touch stimuli should not distort the contour of the skin, but even under these conditions the subjects occasionally localized the stimulus on the opposed part, and in a few of these experiments in which the stimulus was located properly, doubt was expressed of the accuracy of the localization. In other tests stimuli with the camel hair brush were given in such a manner to deform or to exert slight traction upon the skin, and in this way to change the relations or to jar the skin of the opposing part. In these latter tests, however, at no time was the localization of the stimulus reported incorrectly. It is possible that in these latter tests other sensations were evoked besides that of light touch, and that the complex of sensation resulted in the correct localizations. These results are of interest in connection with the phenomena of allochiria, and, in this connection, also, any constant tendency (constant error) in the localization of stimuli near the bodily mid-line may be of importance. Since the investigation of this matter was beyond the scope of the present work, and also of greater importance in relation to abnormal sensations and sensory localizations, its further consideration is left for a future communication.

Localization on a Relatively Insensitive Region.—In the series of tests to determine the constant error, which have already been described, in certain subjects it was found that if, for example, the first stimulus was given on the chest, there was a tendency to locate the stimulus beyond the point stimulated, and in following up each localization and using it as a new stimulus point, the location was shifted towards the axilla. In one of the women who were tested, it was found that in

locating these serial stimuli, she avoided the areola, and the line across the chest was interrupted by a semicircular figure. A stimulus close to the areola of another subject was localized on the areola and close to the nipple. When the latter point was stimulated, she failed to feel the stimulus until the pressure had been very greatly increased. Similar experiments on the other subjects who were tested gave corresponding results. The localization error when the areola was stimulated with the handle of the brush was approximately the same as when the adjacent region of the breast was stimulated in the same manner.

Additional experiments in which the sensibilities of the areola and the nipple were specially tested show that these parts are relatively insensible to light touch stimuli. In no case in which these parts were tested was it found possible to produce a sensation with the light touch of a camel hair brush; and experiments on one subject showed that it was usually necessary to have considerable deformation of the skin before a sensation was obtained. If, on the other hand, the brush was drawn across the areola or the nipple lightly, the subject perceived this stimulation. In such a case, although care was taken to insure only light touch, there may have been traction on the skin, which would deform the part. From all of the experiments it was apparent that the sensibility of the nipple and areola is less than that on surrounding parts, and, although special experiments which would have demonstrated this could not be made, it appeared likely that the light touch sensibility was entirely lacking. It is interesting to know that in tests of this region with cold and hot stimuli, these areas were also found to be less sensitive than the surrounding regions of the breast, the arm, face, leg, abdomen or back. When a warm copper rod was applied to the areola or to the nipple, it was appreciated only as a pressure, although the distinctive temperature character of the stimulus was appreciated when the same stimulus was given to the breast or to the abdomen or arm. Similarly with a copper rod which was cooled, the temperature sensation from this stimulus was not obtained on the nipples and areola, although the temperature effect was immediately noticed on other areas, like the legs, arms and face. In the stimulation of these areas with hot and cold stimuli, the reports of sensations corresponded with those which were obtained by me in the investigation of the protopathic and epicritic sensibilities in a case of nerve division. When a hot rod was laid on the breast across the areola or nipple, wherever this rod touched the skin of the breast, it was felt as "hot," and in the areolar region it was sensed as "warm." When the end of the rod was placed on the areola and the rod was gradually moved over toward the periphery of the breast, the sensation of hotness was reported as soon as the rod left the areolar region. When a rod cooled in a mixture of ice and salt was similarly applied, it was felt to be "cool" in the areolar region and "cold" on the breast and other parts of the body.

The relative insensibility of this part may be "protective" in nature. That it was constantly found in the five subjects indicates that it is probably a normal condition, although, on

the other hand, it is well known that these regions are subject to marked physiological changes of a periodic character.

Summary

1. The average error of localization of light touch varies in different parts of the body, the most accurate localization being found for the face; the succeeding order being: foot, chest, forearm, abdomen, upper leg, upper arm, lower leg, back.

2. The average localization errors with untrained subjects were greater than those found by Ponzo with two trained subjects, but the relative errors were approximately the same.

3. The accuracy of localization of light touch stimuli is greater than that of more intense stimuli.

4. The errors of localization are not constant in direction for the different parts of the body, or on the same part for different subjects, or on corresponding parts for the same subject at different times.

5. The average localization error is less than the double point threshold.

6. There does not appear to be any constant relation between the average localization error and the double point threshold.

7. There appears to be no constant relation between the average lengths of the bodily hairs and the localization error.

8. No practice effects were discovered.

9. Stimuli to a part were sometimes localized on an opposing part, *e. g.*, near the axilla, there were localizations on the chest when the arm was stimulated, and vice versa.[1]

[1] Since the above account was finished, the original articles by Ponzo have come to my notice. Besides the averages which are given in the resumé by Ponzo (*Arch. ital. de biol.*) the only essential material contained in these longer articles is the detailed account of the results of the individual tests.

INNER SPEECH DURING SILENT READING

BY RUDOLF PINTNER

I. Historical Summary

It has long been known that inner speech, during silent reading, plays a very important part in the reading process. Just what the importance of it is, has been variously estimated by different psychologists. Some tend to lay great stress upon it and to maintain that no reading or even thinking is possible without it. Others look upon it more as a habit—a habit that individuals acquire in a greater or less degree. There is little unanimity of opinion and before proceeding to describe some experiments of my own in this field, it will be well to see what attitude psychology has taken and takes toward this important factor of the reading process.

Among the first writers to pay special attention to this phenomenon were the French psychologists Egger[1] and Ballet.[2] Both of these writers approached the subject from the medical point of view. They were primarily interested in investigating the problems of aphasia and they thought and rightly thought that a thorough investigation of the processes inherent in normal speech and reading might throw some light upon the disorders of speech arising in aphasic patients.

Egger found by introspection that inner speech always accompanied his thinking and reading. Or, as he himself says, 'à tout instant, l'âme parle intérieurement sa pensée,' and in regard to reading, "Lire, en effet, c'est traduire l'écriture en parole." With fine psychological insight he notes other occasions when this inner speech is active. When we are troubled by insomnia we cannot 'faire taire notre pensée'—our mind will go on speaking in spite of our desire for silence. Even when we speak aloud, this inner speech is there, accompanying our speaking aloud. In the intervals between one phrase and

[1] Egger, 'La Parole Interieure,' 1881.

[2] Ballet, 'Le Langage interieur et les diverses Formes de l'Aphasie,' Paris, 1886.

another it is preparing the way for what we are going to say next; it acts as 'souffleur' in the intervals. He explains the 'dæmon' of Socrates and the 'voices' of Jeanne d'Arc as merely this inner speech asserting itself with greater insistence than is usual in ordinary individuals. In the same manner, he explains the phrases 'the voice of conscience,' 'the call of duty.' It is merely our inner speech putting into words the thought in our mind and telling us that this is right or wrong, or, that it is our duty to do this or that. The thought of course must be there behind this inner speech all the time, but the voice or call that often seems to us to come from elsewhere is our inner speech that puts into articulate words the vague, indefinite thought. But the voice and call really belong to me, for this inner speech is my speech—'la parole intérieure est comme ma parole.' Only in cases of insanity, as it seems to the present writer, does this inner speech detach itself from the individual and then it appears as some outside being that is talking to the patient; only then is the voice not recognized as one's own.

Ballet, like Egger, relies mainly on introspection and his conclusions are very much in agreement with this latter writer. He brings into the foreground a little more prominently the part that audition plays in reading. He finds that audition is most intimately bound up with inner speech. We not only articulate what we read, but we also hear the words. Which is of most importance and whether we are able to isolate audition from articulation he does not pretend to decide.

Ballet and Egger were not the first writers who noted the importance of suppressed articulation, although it is in their works that we find the subject first thoroughly discussed. Long before the appearance of their works, Bain[1] had noticed this fact, and especially the help that inner speech affords our memory. "A suppressed articulation," he says, "is in fact the material of our recollection, the intellectual manifestation, the idea of speech." And Ribot[2] too, even before Bain, had

[1] Bain, 'The Senses and the Intellect,' 1868, p. 336.

[2] Ribot, 'Les Mouvements et leur importance psychologique,' *Revue philosophique*, Tome VIII., 1879.

noted this fact—"L'homme fait, qui lit silencieusement, accompagner chaque perception visuelle d'un mouvement secret d'articulation." This is of course merely one way in which, according to Ribot, the general principle of motor reaction to every sensory stimulus asserts itself. All psychical processes terminate in movement of some sort or other. If we inhibit the movement then the psychical process is also inhibited, or, as Ribot says, the state of consciousness in question disappears or its character is radically altered. Ribot was not primarily interested in inner speech, but refers to it merely as an example of his general principle of the importance of movement for psychical processes. He is not quite definite as to the part inner speech plays in our general ideas. He does not say conclusively that inner speech is necessary for such general ideas, and he would seem to make an exception in the cases of reflection and meditation, which he supposes may go on without any accompanying articulation.

All these psychologists had relied more or less upon introspection for the facts in regard to inner speech. And so it was with the first German psychologists, who paid attention to this subject, although we soon begin to see the influence of the experimental method creeping in. Stricker[1] relied mainly upon introspection, and indeed he emphasizes throughout his work the fact that all his contentions refer solely to his consciousness. Nevertheless he brings in the introspections of other observers. He asked a hundred persons whether they spoke silently to themselves when thinking or when running over a poem in their mind, and all of these persons answered in the affirmative. We see how Stricker is attempting to get some more general information about his subject, and is not satisfied merely with his own introspection. He found that he could not have the idea of the sound B without having the feeling of some muscular movement or innervation in the lips—"Die Vorstellung des Lautes B und des Gefühl in der Lippen sind also (in meinem Bewusstsein) unzertrennlich assoziiert. . . . Diese Gefühle sitzen in den Muskeln." And as this is true of each separate sound, so it is true for all words. It is impossible, he contends,

[1] Stricker, 'Studien über die Sprachvorstellungen,' Wien, 1880.

for him to have ideas of words without perceiving the corresponding "feelings" (Gefühle).[1] Ideas of words consist of nothing else than of the consciousness of the excitation of those motor nerves that are connected with the articulatory muscles. Ideas of words are motor ideas.[2]

The position here laid down is fairly dogmatic, and although Stricker emphasizes again and again that these observations apply only to himself, yet towards the end of the book his conclusions assume the appearance of universal laws. Ideas of words are in all cases motor ideas.

It was not long before this opinion began to be questioned and criticized. Though not directly concerned about ideas of words, Stumpf[3] concludes quite logically that the same must be true of tones, if Stricker's conclusion is valid. Stumpf opposes Lotze's assumption that no memory of tones or of series of tones is possible without being accompanied by inner speaking or singing. He admits that in many cases there are muscular movements. But the question is whether these are necessary and universal. This he cannot agree with. His introspection shows him that in many cases he makes use of muscular movements in the reproduction or in the judgment of tones, but in quite as many cases he finds that he does not. "Ohne lautes, leises oder stilles Singen kann ich verschiedene Töne vorstellen." And this statement he supports by the introspection of other musicians. Again if Stricker's theory were right, every difference in tones that can be distinguished would correspond to a difference in muscular movement. And this is scarcely credible when one reflects that many musicians can distinguish tones that lie one oscillation and less apart. Stumpf argues the case further, but we need not follow him, for this latter argument is not quite convincing.

Stumpf was not alone in his opposition to Stricker. Paulhan[4] opposes the latter and holds that he can have an image of a vowel (probably an auditory one), while pronouncing aloud another vowel—a thing that Stricker held to be impossible.

[1] Stricker, op. cit., p. 16.

[2] Stricker, op. cit., p. 33.

[3] Stumpf, 'Ton-psychologie,' 1883, p. 154 et seq.

[4] Paulhan, 'Le langage intérieur et la pensée,' *Revue philosophique*, XXI., 1886.

Paulhan points out that the latter writer had laid too much stress on motor imagery, and suggests that perhaps the attention paid to it had called it into being in a great many cases. Now Paulhan found this to be the case, and he got motor images principally when he was paying special attention to the process of inner speech. There are two distinct elements, the auditory and the motor, and the auditory may exist without the motor. Whether the motor may exist without the auditory, Paulhan does not pretend to determine. Inner speech is a complex phenomenon made up of visual, auditory, motor, tactile and abstract elements. Each of these classes of elements may predominate in different individuals. Thought is an inner language, which need not be reduced to words or images of words; and again "la pensée est un langage, non une parole, et, si la représentation des mots lui est utile, elle paraît, de son côté, faciliter beaucoup cette représentation."

Baldwin[1] agrees with Paulhan in finding it possible to think different notes very clearly while the vocal organs are held rigid. He asserts his ability to think one note while uttering aloud a long drawn-out vocal sound, say ā, in a different pitch. He finds moreover that when his attention is directed to the larynx, the movements appear, whereas if his attention is directed to the ear, these movements fall away or disappear. And this he holds is an indication of two speech types—a sensory and a motor type.

But the opposition to the views expressed by Stricker and Bain is not yet finished. We find Bastian[2] and Collins[3] opposing them from the more physiological point of view, and the latter brings instances from the disturbances of aphasia to support his contention. He cites a case of cortical motor aphasia, in which the internal speech was gone, but the patient could still read. If this is so, then it would seem to indicate that articulatory movements are not necessary.

With this we are approaching the whole question of a muscular sense and the much debated sensations of 'innerva-

[1] Baldwin, 'Mental Development in the Child and Race,' 1895, p. 442.

[2] Bastian, 'Brain as an Organ of Mind,' 1891, p. 595 et seq.

[3] Collins, 'The Faculty of Speech,' 1898, p. 62 et seq.; p. 195 et seq.

tion.'[1] A consideration of these problems would lead us too far away from the muscular movement involved in reading. We shall therefore restrict ourselves to those investigations which bear more directly upon our problem.

Dodge[2] made an introspective study of verbal imagery. He found that silent thinking was for him mainly an inner speaking; that the characteristic elements of the words themselves were reproductions of the movement-feeling which arises in actual speaking, derived mostly from the lips, tongue and throat, less clearly from the breast and thorax. Actual sensations coming from the periphery, and caused by articulatory movement are not essential for inner speech; their reproduced elements are sufficient. We see here that much less emphasis is laid upon the actual sensations of movement, and that Dodge's evidence would therefore also oppose the contention of Stricker and Bain, who suppose that actual movement must be present. And yet the ideas or images of movement are necessary. And this view is supported also by Bawden.[3] He holds that we must have motor or kinæsthetic ideas. These ideas are necessary for the meaning of words. "The meaning ideas which constitute the content of our verbal association are ultimately kinæsthetic or motor, not visual or auditory. There must be this return wave of the kinæsthetic imagery to select and organize the visual and auditory perceptions before they have any meaning." From this it would seem that actual articulation is not necessary, but that the idea of the movement in some form or other must be present in order that the meaning of the word should come into consciousness. Reading then without actual articulation should be possible.

Let us see then what experimentation in this field leads to. If Stricker and Bain are right then inhibition of articulatory movements should arrest the reading process, that is, the eye will move over the words but the meaning of the context will not rise into consciousness. If the numerous other writers are

[1] See Wundt's 'Physiologische Psychologie.'

[2] Dodge, 'Die motorischen Wortvorstellungen,' Halle, 1896. Review by Delabarre in Psych. Rev., 1897.

[3] Bawden, 'A Study of Lapses,' Psych. Review, Mono. Supp., III., No. 4, 1900, p. 154 et seq.

right, then such inhibition should not absolutely prevent the possibility of getting some meaning from the context. To prove this second contention, it is not necessary that we read as well as usual during inhibition. If we do not read as well it will only show that actual articulatory movements form a customary part of the reading process but not a necessary part. It will show that they are habitually there, but it will also prove that the reading process can dispense with them. Whether their elimination is desirable or not can only be shown by experiments, which prove that reading can be carried on as quickly and as satisfactorily without them.

If we turn now to these movements themselves, we must consider whether they are present in all cases of ordinary reading and how marked their appearance is.

In regard to the second point, there are interesting observations to show that these movements may be present without the observer himself being conscious of them. In this respect the experiments of Hansen and Lehmann,[1] undertaken to investigate so-called thought-reading, are very illuminating. These experimenters found that when thinking intently of some number or word an unconscious whispering nearly always occurred, and that this whispering could actually be heard by an observer placed in a position where the acoustics were particularly favorable. The mouth of the whisperer was tightly closed and an onlooker could observe no lip-movements. Now it is impossible to whisper however softly without calling the muscles of articulation into play, and so these experiments show us how slight and difficult to observe such movements of articulation may be, and that these movements may be present without the individual himself being aware of them. We should be very careful therefore in asserting from introspection alone that these movements are in any given case not present. It will not do to assert, as Baldwin did, that we can think of some word when holding the articulatory muscles rigid. We may think we are holding them rigid, but we may be at the same time unconsciously moving them.

[1] Hansen und Lehman, 'Über unwillkürliches Flüstern,' *Wundt's Phil. Stud.*, XI., 1895.

Now although these movements may be very slight and imperceptible, as Hansen and Lehmann have shown, this is not generally the case. Other experimenters have taken records of these movements when the subject was reading or reciting mentally. Curtis[1] placed a large tambour on the larynx and took a record of the movements. He contrasted normal curves, *i. e.*, curves taken while the subject was thinking of nothing, with curves taken when the subject was thinking of something definite or when reading silently. The latter curves nearly always showed large movements, much larger than when the subject was trying to think of nothing. He experimented with twenty observers and fifteen of these showed marked movements, the remaining five showed no movement, but they also showed no movement when whispering in an audible manner. The conclusion from these five is a negative one. In all probability articulatory movements were present, but the instruments used were not delicate enough to record them. Again the larynx is not the only organ brought into play by articulation, and it is possible that in many cases the anatomical build of the neck prevents direct movements of the larynx being recorded.

Courten[2] experimented upon another organ that is brought into play by articulation, namely, the tongue. By means of a Rousselot exploratory bulb placed upon the tongue and connected with a Marey tambour, he was able to get a record of any movement of this organ. In the same manner as Curtis, he compared curves taken during a period of mental rest with those taken when the subject was reading or thinking out a special problem. He found that the amount of movement differed with the individual and also with the matter read or thought about and with the degree of attention concentrated upon the matter. But he states definitely that no record was taken which did not show some movement.

All these experiments would go to show that in all cases, as far as we can tell, articulatory movements are present in

[1] Curtis, 'Automatic Movement of the Larynx,' *Amer. Jour. of Psych.*, XI., 1899.

[2] Courten, 'Involuntary Movements of the Tongue,' *Yale Psych. Studies*, Vol. X., 1902.

ordinary reading and thinking. The experiments are few and do not warrant us in laying down a general law. They cannot be considered as proving that articulation is always present, but the probability that it is present is very great. We dare not, however, argue from this that it must be present in every case.

If we turn now to those experiments that have attempted to measure the effect upon our memory-power or our reading of an inhibition of the movements of articulation, we find that nearly all observers agree that such inhibition decreases our ability to remember and to read. Münsterberg's[1] device of employing the musculature of articulation during the attempt to memorize has generally been used. W. G. Smith[2] asked the subject to repeat a series of numbers, 2, 4, 6, etc., or 3, 6, 9, etc., to the rhythm of a metronome while attempting to memorize syllables or words. Another device he used was the repetition of the syllable 'la'. Th. L. Smith[3] used as a distraction the device of counting aloud 1, 2, 3, 1, 2, 3, etc., during the memorizing. These experimenters were mainly concerned with the effect of such distraction upon the memory. They all found that the memory was thereby hindered in its usual attainment. W. G. Smith found that a great loss of memory was due to repeating a syllable in an audible voice, and that in the intervals of auditory articulation, an attempt is often involuntarily made to insert an inaudible articulation. Th. L. Smith found that every subject showed an increase of error, varying from 12.6 to 17.7 per cent., due to the introduction of counting.

In his exhaustive study on inhibition, Breese[4] tried some memory tests to compare a normal series with a series memorized while the breath is held. In such a series the memory was found to be worse.

The general conclusion from all such experiments seems to be that articulatory movements are always present and that

[1] Münsterberg, 'Die Association successiver Vorstellungen,' *Zeitschr. f. Phys. und Psych.*, I., 1890.

[2] W. G. Smith, 'The Relation of Attention to Memory,' *Mind*, 1895.

[3] Th. L. Smith, 'On Muscular Memory,' *Amer. Journ. of Psych.*, Vol. VII., 1896.

[4] Breese, 'On Inhibition,' Psych. Rev., Mono. Suppl., Vol. III., 1899.

any interference or interruption of such movements disturbs the normal processes. The only experimenter who claims that this process is not always present is Secor.[1] He mentions that one of his observers had no articulation in ordinary reading, but for this statement he relies upon the introspection of the observer himself. We have shown how misleading this may be, for as we have already stated articulation may be present and the subject himself may be entirely unconscious of it. In face of the large amount of testimony to the contrary, we cannot accept the statement of Secor's observer that he had no articulation during reading.

But Secor's experiments are interesting in as much as he tried various methods of inhibiting articulatory movements. All his subjects found that saying the alphabet aloud or whistling completely removed all traces of articulatory movement. Such devices were not merely a distraction of attention, for clapping the hands during reading left the articulation quite clear and allowed the observer to read at his ordinary rate. Secor does not give the amount of disturbance that was caused in the reading process by inhibiting articulation. He is mainly concerned with the question whether articulation can be inhibited. He further attempted to shut off audition and found that when a xylophone was played near the observer it had the effect of inhibiting audition in some observers but not in others. I do not think that any such method of suppressing audition can be relied upon. Most people can read and hear the inner auditory accompaniment quite plainly even in the midst of a great noise. Any one can try this for himself. The noise or music may be a distraction to the attention, but whatever reading is done is always accompanied by inner audition.

The general result from all these experiments can be summed up by saying that silent reading is accompanied by articulation in some degree or other. This activity of articulation is, so far as we know, a universal habit. Whether it is a necessary habit is another question. We have seen that theoretically many psychologists believe that it is not neces-

[1] Secor, 'Visual Reading,' *Amer. Jour. of Psych.*, Vol. XI., 1899.

sary, that in fact we can think and read without articulation. It would be the business of some experiment to prove that it is not necessary and to prove it by showing that we can by practice acquire the habit of reading without articulation and that such reading can be as rapid and as meaningful as reading with articulation. Huey[1] believes that this articulation hinders reading and that it would be desirable to get rid of it. "The direct linking of visual form to ideas, cutting out of the circuit the somewhat cumbrous and doubtless fatiguing audito-motorizing mechanism, would seem to be a consummation to be wished for." Again and again it has been pointed out that the articulatory movements act as a drag upon the rapidity of the reading process. Dearborn[2] believes that the eye can move much faster and that the attention is able to grasp more, but that both are hindered by articulation. "The effect of articulating is to decrease ordinarily the span of attention." Quantz[3] found that the slowest readers show most lip movement. And lip movement of course means that the articulation is very marked. Abell[4] came to much the same conclusion —"a characteristic correlate of slow reading in the case of our subjects is the actual pronunciation or the vivid articulatory imagination of the words read." That is to say, that the faster readers tend to slur over the process of articulation more and more, and as rapidity in reading may coëxist with marked ability of comprehension and assimilation, this process of articulation appears more in the light of a hindrance than anything else.

II. The Experiments

The experiments undertaken by the writer and described below were an attempt to find out whether this process of articulation could be eliminated, and if so whether by practice the ordinary rate of reading and the average degree of comprehension could be attained.

[1] Huey, 'The Psychology and Pedagogy of Reading,' 1900.

[2] Dearborn, 'The Psychology of Reading,' *Archives of Philosophy*, Vol. 4, 1906.

[3] Quantz, 'Problems in the Psychology of Reading,' Psych. Rev., Mono. Suppl., Vol. II., 1897.

[4] Abell, 'Rapid Reading,' *Educational Rev.*, Vol. VIII., 1894.

The experiments were carried out at the University of Chicago during the summer term, 1912. Mr. Culver and Mr. Smiley acted as observers and my best thanks are due to them for their help.

The method employed was to make the observer read over certain selected passages of prose while at the same time inhibiting articulatory movements. Many different methods of inhibition were tried over on myself—such as trying to hold the muscles rigid,[1] whistling,[2] singing a note,[3] repeating la, la,[4] etc., or a, b, c, a, b, c,[5] etc., or 1, 2, 3, 1, 2, 3,[6] etc. I finally came to the conclusion that the repetition of some word was most satisfactory and it seemed to me that this word should not be such a simple syllable as 'la.' During the repetition of such a syllable, I believe that certain articulatory movements can take place. The long drawn-out vowel sound and the open position of the mouth seem to facilitate some slight movements. I observed marked fluctuations in the pitch of the sound la, when drawn out for some time, which may be due to slight muscle movements caused by the reading process. At any rate I thought it best to employ the repetition of some numerals, and as 1, 2, 3, 1, 2, 3, etc., seemed to me not quite complex enough as to sound, I selected 13, 14, 15, 16, 13, 14, 15, 16, etc. A very short period of practice in counting such a series aloud served to make the process quite mechanical. The results of the experiments bear this out. At first there were many hesitations and interruptions, but gradually the process became automatic. In thus employing the musculature of articulation, I believe with Secor that we are shutting out the possibility of any articulation of the words read at the same time. Whether we shut it out absolutely, it is difficult to say. It may be that in the spaces between the articulation of one syllable and another, some slight movement may creep in to help the silent reading; or it may be that help

[1] Baldwin, *op. cit.*
[2] Secor, *op. cit.*
[3] Stumpf, *op. cit.*
[4] Baldwin, *op. cit.*
[5] Secor, *op. cit.*
[6] Th. L. Smith, *op. cit.*

is gained when the movement of articulation of the word pronounced aloud corresponds to the movement that would be necessary for the word read silently. Just how much help is thus gained and indeed whether any such help is thus obtained, it is with our present experimental methods impossible to say. If we rely upon the introspection of the observers, we may say that by this method inner or silent articulation is prevented.

In regard to the ever-present accompanying audition, no attempt was made to exclude this, for the simple reason that there seems to me at present to be no satisfactory device that shuts out audition. Secor's device of playing some musical instrument near the observer is quite unreliable. It will not shut out audition. Most of his observers were not disturbed by it. In my experiment, therefore, audition remains undisturbed. When a satisfactory method has been devised to shut out audition, interesting results could no doubt be obtained.

Now in judging the value of the reading of any passage, two factors are to be taken into account: (1) rapidity, (2) comprehension. The best readers are those who read most rapidly and at the same time comprehend most. It will not do to leave any one of these two factors out of account. Great rapidity in reading is undesirable, if accompanied by a poor ability to comprehend the matter read. Although many experimenters have found that rapid readers are often the best at comprehension, yet the mass of evidence is not sufficient to take this for granted. Romanes[1] was one of the first to point out this fact, yet he himself mentions the case of many distinguished literary men who are slow readers. Abell[2] found that the moderate readers showed the best comprehension, and that slowness of reading does not follow from slowness of comprehension. Hence it would seem necessary to have a check upon the amount comprehended as well as upon the speed of reading. And with this object in view I prepared the material to be read.

I cut out over one hundred short passages from copies of

[1] Romanes, 'Mental Evolution in Animals,' p. 136 et seq.

[2] Abell, op. cit.

Munsey's and the *American Magazine.* Each passage was selected so as to contain ten definite ideas, which we shall call points. The same method of analyzing matter into points has been employed in memory investigations by Binet and Henri,[1] and by Shaw.[2] Binet and Henri divided a sentence as follows: Le petit Emile / a obtenu / de sa mère / etc. Shaw divided the sentences in the same manner, *e. g.*, James / Mack / ten years old / a farmer's son / etc. This division was no doubt excellent for memory experiments, where the greatest emphasis is laid upon the number of actual words retained. In testing the comprehension of reading less stress must be laid upon a reproduction of the actual words. It suffices if the general thought of the passage has been grasped. My divisions therefore included much more within a single point, and this point was regarded as comprehended if the same idea was reproduced, regardless of the similarity or dissimilarity of the actual words read. Here is an example of one of my passages divided up into ten points: "Finding that graft and corruption were debauching the city / —not, alas, for the only time in its history / —he helped to organize a fight / to clean them out. / When neither of the old parties would recognize him, / the Populists took him up / and he was elected mayor. / He was a determined foe of entrenched privilege / and did much for the people / of San Francisco." /

All the passages began with the beginning of a sentence and ended with the end of a sentence or phrase.

In regard to the number of words in each passage, it was from the nature of the case impossible to keep this absolutely uniform. As far as possible this was done, and it was found that the desired number of passages could be obtained with an average number of about 70 words. The shortest passage contained 60 words, and the longest 79. From this it will be seen that the passages were fairly short and this seemed desirable, in as much as it would tend to make the passages more uniform, and would decrease the amount of work entailed in apportioning marks to each passage.

[1] Binet et Henri, 'La mémoire des phrases,' *L'année psychologique*, Vol. I., 1894.

[2] Shaw, 'Memory in School-children,' *The Pedagogical Seminary*, IV., 1896.

All the 103 passages were carefully analyzed and the ten points contained in each passage were written down to form a kind of objective standard by which to judge the observer's work. It was from this record that marks were apportioned to the work of the observer. By this means the fluctuations of subjective judgment were minimized.

After the passages had been thus analyzed they were pasted on pieces of gray cardboard, 11.5 by 6.5 cm. They were then numbered and arranged in three sets. The first set of twenty was used for ordinary reading, the second set of sixty-three (seven groups of nine each) was used for reading with suppressed articulation, and the last set of twenty for ordinary reading after practice with inhibition.

The Arrangement of the Experiment.—The observer sat comfortably in a chair in front of a table upon which a card with reading matter was placed. This card was covered over with a larger sheet of gray cardboard. On this was a fixation point which marked the place of the reading matter underneath. Previous trial cards had been shown to the observer so as to enable him to determine the most comfortable distance of the eye from the reading matter. When the observer was ready he gave a signal to the experimenter by tapping on the table. The experimenter then removed the large sheet of cardboard with one hand, thus exposing the reading matter to view, and with the other hand set a stop-watch in motion. As soon as the observer had read the passage he signalled again to the experimenter who covered the reading-matter and stopped the watch. The observer then wrote down as much of the material as he could, and the experimenter recorded the time. After some trials had been taken and the method of procedure thoroughly mastered, a first series of twenty readings was taken. The observer was told to read at his normal rate, but to do his best at each reading. I did not deem it advisable to lay too much stress upon the rapidity of reading. I believe that the very fact that the observers were being timed would lead them to read as rapidly as possible compatible with an intelligent understanding of the matter read. The writing down of the sense of the passage after each reading kept before

them continually the necessity of reading not only quickly but also intelligently.

In the second group of readings the observer repeated in an audible voice 13, 14, 15, 16. Practice in this preceded the actual experiments. All the other conditions of the experiment remained exactly the same. Sixty-three such readings were taken, allowing a division of them into seven sets of nine each, so that a median for each set could be taken. The third group consisted of a set of twenty readings carried out without inhibition exactly the same as in the first set of twenty.

The experiments were conducted as regularly as possible every day at the same hour. No definite number of passages was assigned for each day, but it was found that seven or eight was a good limit, beyond which fatigue or restlessness was likely to set in.

The Results.—The following method of evaluation for each passage was adopted. Suppose the time taken to read a certain passage was 18.3 seconds and the number of correct points gained in that passage was 7.5, *i. e.*, the observer assimilated and reproduced three fourths of the total passage. If we divide this 75 per cent. reproduced, by the time, 18.3 secs., we get a value of 4.09, which represents the percentage of points assimilated per second. This value I have called the "reading value" of a passage. The shorter the time and the larger the number of points the higher will this value be, and vice versa. The best readings then, taking into account assimilation and rapidity, were denoted by a high reading value, the worst readings by a low value.

We thus obtain a reading value for each passage read, and in glancing over these values, we find that there is little uniformity in them. This can be seen clearly from the curves I.–IV., which show the reading values for each test with and without inhibition. For example in curve I. we see the reading value fluctuating from .8 to .6 to 1.3 and so on, and larger fluctuations can be noticed in other curves.

These fluctuations are without doubt due to the differences in the reading matter. The difficulty of each passage, in spite of all efforts to the contrary, is certain to differ greatly. The

familiarity of the subject-matter to each individual reader is also a source of fluctuation. Certain passages will appeal to a reader much more than other passages. It may be that he is especially interested in the subject matter, or it may be that he has been reading about that subject shortly beforehand.

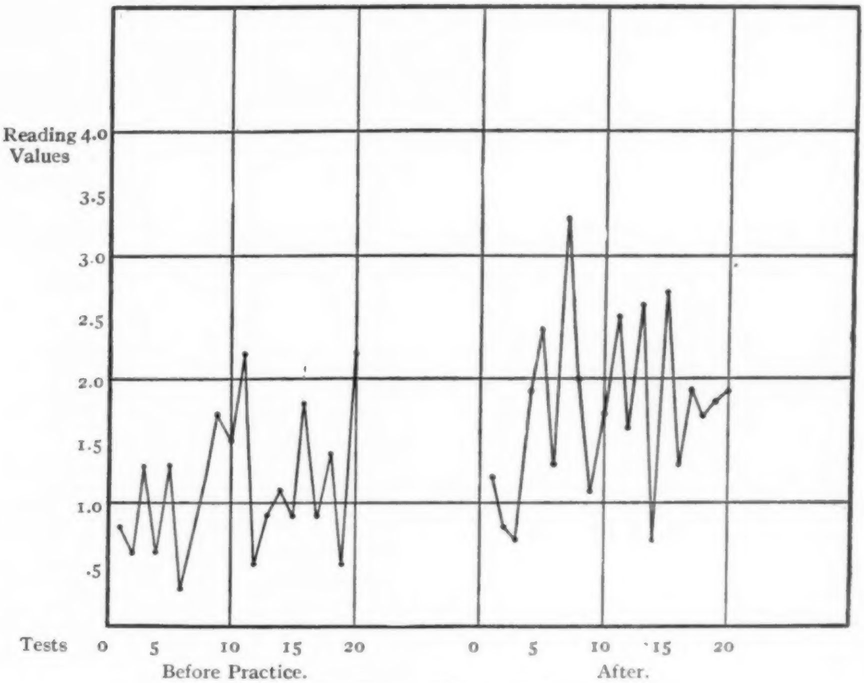


FIG. 1. Obs. *A*. Curve of Tests. Ordinary Reading.

Another passage may awaken no interest. Observer *B* for example expressed an interest in passages dealing with baseball or in those referring to the life of Houston, a period of history with which he was well acquainted. On the other hand he expressed no interest and no special knowledge of aviation. In four passages dealing with this latter subject his reading values lay between .8 and 2.0. In four passages dealing with baseball or with Houston his reading values lay between 2.2 and 6.0. This shows clearly what an influence interest or knowledge of a subject exerts upon the reading value. Such large differences do not invariably appear, but

they tend to do so. One passage observer *B* recognized as belonging to the story "Marriage," by Wells, a story that he had read, but in spite of this his reading-value for this passage without inhibition was only 2.2, considerably below his average performance. We cannot therefore say dogmatically that a

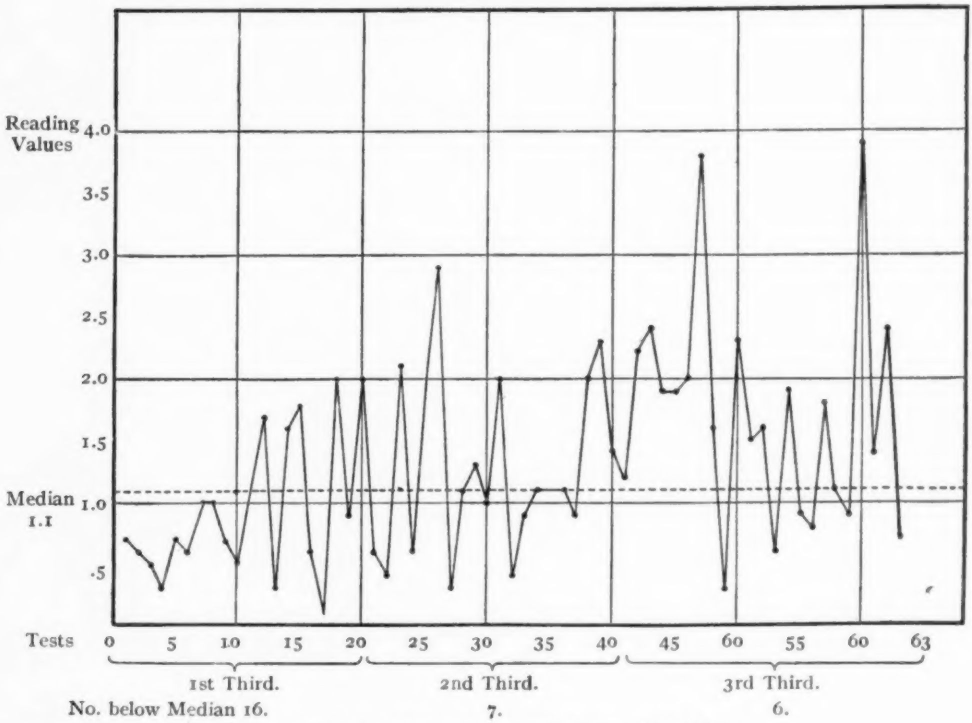


FIG. 2. Obs. *A*. Curve of Separate Tests. Inhibition.

previous reading or knowledge of the context or special interest necessarily increases the reading-value. It tends to do so in most cases and helps us to explain the fluctuation in value from passage to passage.

An examination of curves I.–IV. gives us therefore little direct knowledge of the increase or decrease of reading-value. The curves are too unsteady to show that at a mere glance. If, however, we take the medians of certain groups we get figures that will tell us whether progress is being made or not. For this reason I divided the 63 passages read with inhibition

into seven groups of nine and took the medians of each group. I also took the median of the twenty readings before and after inhibition. This is shown in Table I. In the first column are the different groups from which the medians were taken, seven

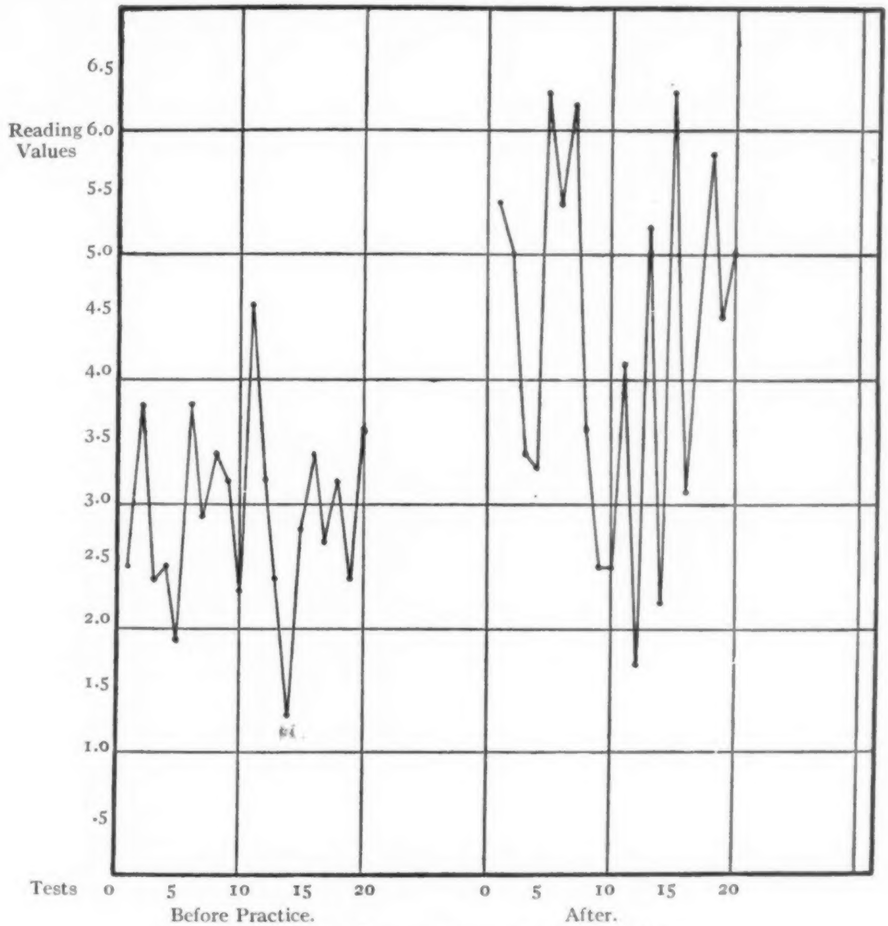


FIG. 3. Obs *B*. Curve of Tests. Ordinary Reading.

with inhibition and two with ordinary reading, one before and one after practice with inhibition. In the second column the medians of the actual time taken to read the passage and in the third column the median reading value. These facts are given for both observers.

Table I

Summary of Medians

Observer A			Observer B		
Groups	Time	Reading Value	Groups	Time	Reading Value
Ordinary before....	31.6	1.0	Ordinary before....	18.0	3.0
Inhibition			Inhibition		
Group *A*........	31.6	.7	Group *A*.........	22.9	1.1
" *B*........	27.2	1.0	" *B*.........	20.0	2.0
" *C*........	26.6	.9	" *C*.........	18.7	2.6
" *D*........	31.2	1.1	" *D*.........	18.3	2.6
" *E*........	24.2	1.9	" *E*.........	15.3	3.2
" *F*........	28.3	1.6	" *F*.........	15.4	3.2
" *G*........	28.3	1.1	" *G*.........	12.1	3.9
Ordinary after.....	26.0	1.8	Ordinary after	12.0	4.0

A glance at this table shows that the time tends to decrease as practice in reading with inhibition is gained. An absolutely steady decrease is noted with Obs. *B*. An increase in reading value is also noted. This is rather unsteady but nevertheless marked with Obs. *A* and again absolutely steady with Obs. *B*. Another interesting fact is that in both cases the median reading value after practice in inhibition is greater than the median previous to such practice. In both cases again do the first few median values with inhibition drop below the median of ordinary reading before practice in inhibition, and again in neither case does the highest median with inhibition exceed the median of ordinary reading after inhibition, with the exception of 1.9 in group *E* of Obs. *A*, where it exceeds the ordinary median 1.8 by .1.

Now the fact that the reading value increases so steadily seems to show that reading without articulation makes great progress through practice. The same progress can be seen on curves II. and IV. On these curves the general median for all the 63 tests taken together has been marked with a dotted line. If now we divide the tests into three thirds and count the number of cases where the reading value falls below the median in each third, we find that for Obs. *A* these numbers are 16, 7 and 6 for the three thirds, and for Obs. *B* 18, 11, 2, showing an increase in the reading value as practice in reading without articulation is gained. I believe that the curves show actual practice in reading without articulation, and that this increase

is not merely due to the fact that the observer is adapting himself better to the conditions of the experiment. Certainly

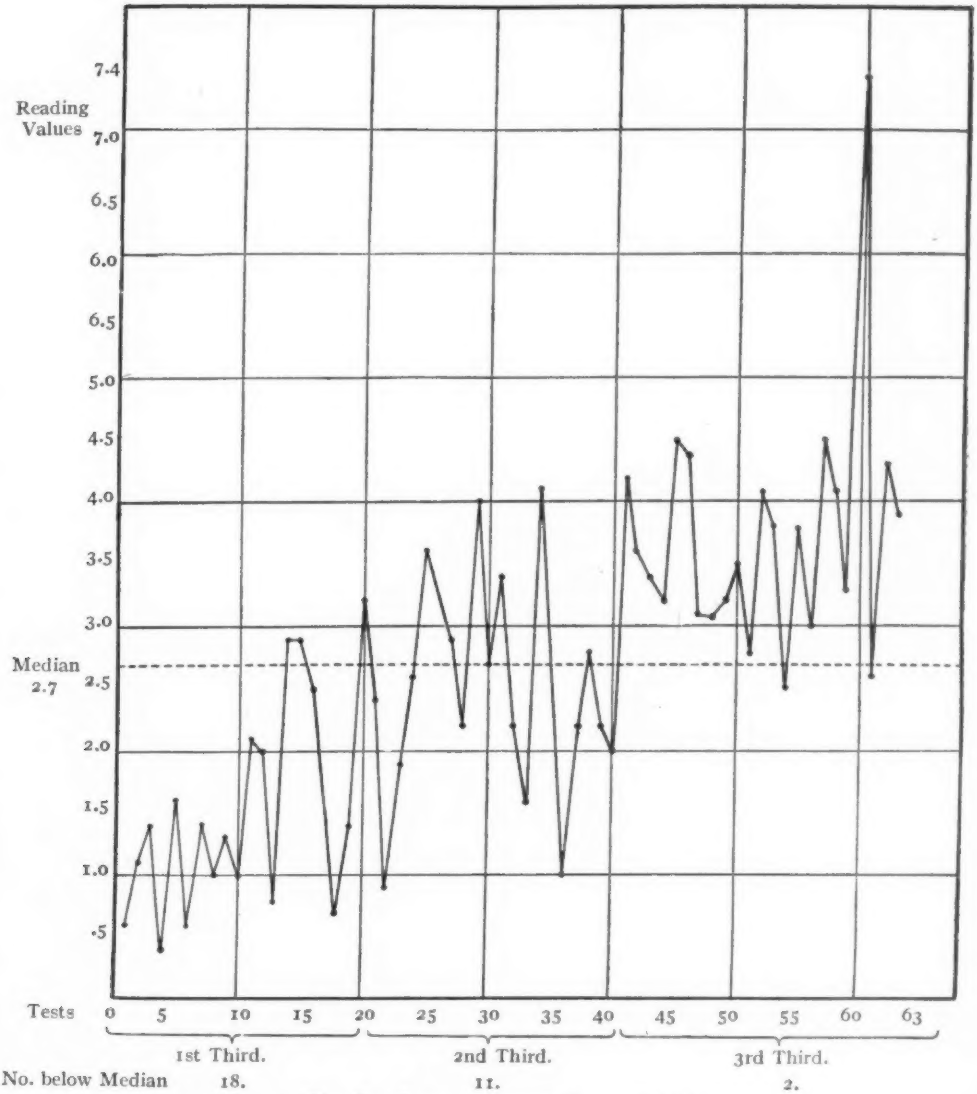


FIG. 4. Obs. *B*. Curve of Separate Tests. Inhibition.

the observer has in the first place to overcome the distraction caused by dividing his attention between the reading process

and the counting aloud. But this counting aloud soon becomes automatic and little attention is needed for it. If the reading were to suffer merely because the attention is directed to something else, then this should show itself when the attention is directed to some other movement than counting aloud. I tried clapping the hands during silent reading and found that

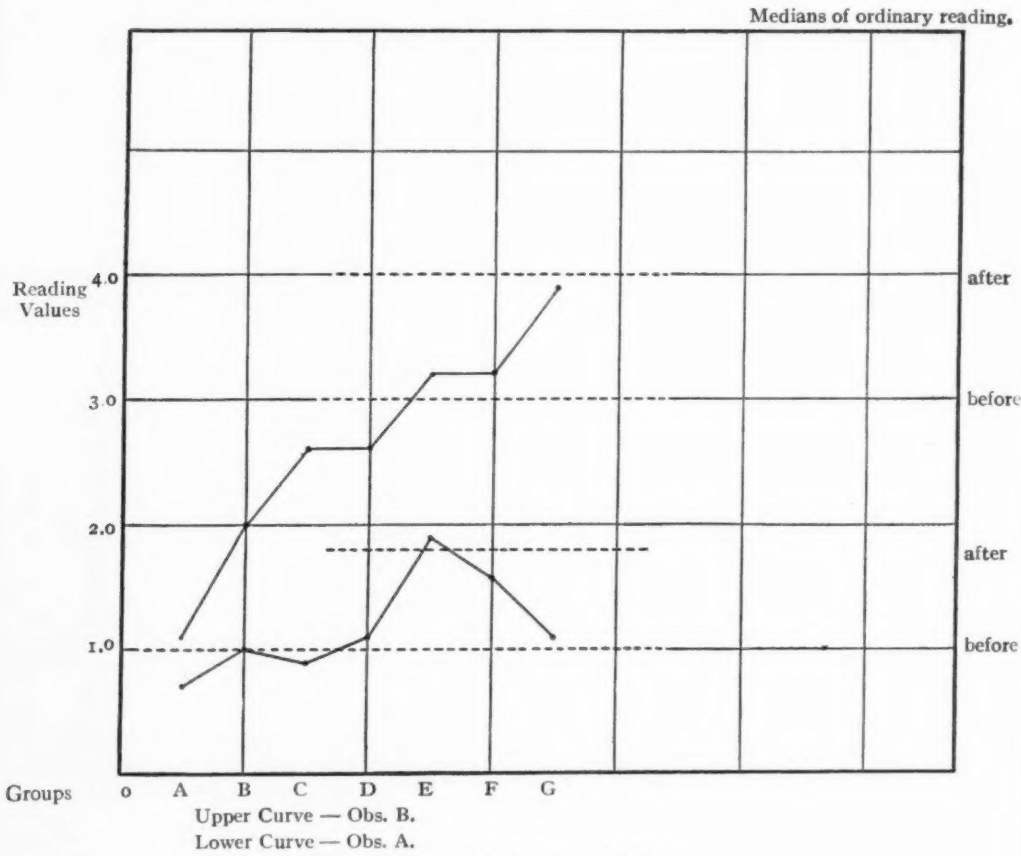


FIG. 5. Curves of Medians. Inhibition.

division of attention in this case had no effect upon the reading value. In the same way therefore I believe that the division of attention in counting aloud is not the prime factor, a better mastery of which alone causes the reading value to increase.

That the process of counting aloud soon becomes automatic

can be seen to some extent in the number of hesitations and interruptions that occur in the counting. A record of these was kept. Observer *A* decreased from 18 such hesitations or interruptions in group *A* to 7 in group *G*; observer *B* from 13 to 1 respectively. This seems to show that the counting is becoming automatic and that the reader is inhibiting more and more successfully the strong habitual tendency to articulate.

From the above results I think we are justified in saying that reading without articulation can take place, that it can be accomplished as quickly as reading with articulation and that practice in reading without articulation increases the ordinary rate of reading, no doubt due to the fact that after such practice the amount of articulation made use of is not so great as formerly. The observer knows that he can get along with no articulation and therefore he very probably articulates much less and not so carefully as before. This is shown in both observers. Their value for ordinary reading was greater after practice than before. In both cases the observers were aware of a return to articulation during ordinary reading after the series in suppressed articulation. It would seem then that such short practice as was possible in these experiments is not enough to eradicate a deep-seated, life-long habit of articulation during reading and of course this is not to be expected. But that this practice led to a decrease in the amount of articulation used immediately after practice, I am sure, and this is further strengthened by the statement of observer *B*. Towards the end of the series with inhibition, he made the introspection that he was not reading all the words, which seemed to him to be his custom in ordinary reading but that his eye seemed "to skim over the words." After the twenty tests in ordinary reading, in which his reading values were greatly increased, he asserted again that he seemed here to be skimming over the words, depending mainly on his eyes, as in the previous tests. He was not, of course, depending absolutely on his eyes, because movements of articulation were sometimes noted—lip movements and movements of the muscles of the larynx.

In these experiments no attempt was made to control or

regulate the rate of counting. I deemed it best to allow each observer to fall into his own rhythm, believing that thereby he would naturally adopt the rhythm most advantageous to himself and so distract his attention as little as possible. I do not think that the rate of counting affects the rate of reading although no special experiments were made to test this. I noted the average time it took the observer to count 13, 14, 15, 16. Observer *A*, the slower reader, took .5 sec.; observer *B*, the faster reader, took .9 sec. The fast counter then is not in this case the fast reader.

As regards the introspections of the observers it may be noted that both were absolutely certain that no articulation was possible during the counting aloud. As to audition, they were not so sure. Asked as to whether anything else was going on when the articulation was suppressed, observer *A* remarked "Something up in the head," and later on he identified this "something" with audition. Observer *B* thought that no audition was present in his case. Both observers agreed in being certain they were getting no help during inhibition of articulation by means of movements of the fingers or body. Again both observers said that in writing the passage from memory they derived most help by visualizing as much as possible the scene and circumstances of the passage. Observer *B* tended at times to visualize the actual words.

No instructions were given to the observers in regard to the way in which they were to write down the passage. Observer *A* always began at the beginning and wrote straight on, making few alterations or corrections. Observer *B* sometimes adopted this method, but very often he jotted down the last sentence, that was still fresh in his mind, and then began to work down to this sentence from the beginning. He would also occasionally jot down a proper name immediately after finishing the reading. These aids to memory, which *B* adopted more than *A*, would account in part for his much higher reading values.

In summing up the results of these experiments the following are the most important:

1. That articulation during the reading process is a habit, which is not necessary for that process.

2. That practice in reading without articulation can make such reading as good as the ordinary reading of the same individual.

3. That practice in reading without articulation tends to aid ordinary reading, most probably by shortening the habitual practice of articulation.

These experiments cannot answer the question as to whether articulation can be entirely eliminated from the reading process. The amount of practice obtained by the two observers was not sufficient to prevent them from falling back into the habit of articulation. It is questionable whether such a habit, that is of such long standing and so deeply rooted in the adult, can be permanently overcome.

OBTAINING THE MEAN VARIATION WITH THE AID OF A CALCULATING MACHINE

BY KNIGHT DUNLAP

The Johns Hopkins University

Finding the mean variation of a series of values by the method usually employed[1] is a laborious process, and unless done at the expense of relatively great time and pains, is apt to involve errors. No reliance is commonly placed on the mean variation unless it is calculated from the *plus* and *minus* variations separately. Any one who has done much of this work knows how wearisome it is. By the use of a calculating machine, great accuracy may be obtained with the minimal expenditure of time and energy, if some method is followed which does away with the subtractions of the old method; for each subtraction requires the "setting up" of two numbers on the machine, which makes the old method slow and troublesome.

The method which we have developed in the Johns Hopkins laboratory is so simple that it seems improbable that it has not been in use before this; but as it is new to us and to many others, it seems worth while to describe it. I shall give first the mathematical justification of the method, and the rules for following it, and then describe the operation on the machine.

In a given series of N terms, with average M, there are P terms greater in value than the average, and R terms less than the average. Let ΣP be the sum of the terms greater than the average, and ΣR be the sum of the terms less than the average. Let p be the difference between the average and any of the P terms, and let r be the difference between the mean and any of the R terms, the differences being all taken with

[1] Myers, 'Text Book of Experimental Psychology,' 1st ed., 367–368; Sanford, 'Experimental Psychology,' 346; Titchener, 'Experimental Psychology, Quantitative,' Student's Manual, 8, 182; Külpe, 'Outlines of Psychology,' Titchener's translation, 50; Wundt, 'Physiologische Psychologie,' 6te Aufl., III., 392.

plus signs. Let Σp be the sum of all the P differences and Σr be the sum of all the R differences.

From these definitions it follows that:

$$\Sigma P = P \cdot M + \Sigma p \tag{1}$$

and

$$\Sigma R = R \cdot M - \Sigma r. \tag{2}$$

And from the definition of an average, it follows that:

$$M = (\Sigma P + \Sigma R) \div (P + R); \text{ or, } \Sigma P + \Sigma R = M(P + R); \tag{3}$$

from which three equations it follows that $\Sigma p = \Sigma r$.

Now by definition of the mean variation it is $(\Sigma p + \Sigma r) \div N$ which is the same as $2\Sigma p \div N$, or $2\Sigma r \div N$, and from (1), $\Sigma p = \Sigma P + P \cdot M$; and from (2), $\Sigma r = R \cdot M - \Sigma R$; therefore,

$$MV = (\Sigma P - P \cdot M) \div \tfrac{1}{2}N = (R \cdot M - \Sigma R) \div \tfrac{1}{2}N.$$

The alternative rules, in accordance with the formulæ, are:

1. Add together the terms greater than the average; from the sum subtract the product of the number of terms (P) so added, multiplied by the average (M); and divide the remainder by half the total number of terms in the series ($\tfrac{1}{2}N$).

2. Add together the terms which are less than the average: subtract the sum from the product of the number of terms (R) so added, multiplied by the average (M); and divide the remainder by half the number of terms in the total series ($\tfrac{1}{2}N$).

If the process is carried out on the calculating machine (and it is not economical otherwise), the second rule is the simpler. If the machine prints, only the one calculation is needed, as the chance of error is practically zero. If a non-printing machine is used, it would be better to work the result out by both rules: if the two results are the same, it is practically certain that not only the derivation of the MV from the average, but the derivation of the average itself, has been correctly done.

The process, as described, seems complicated, but in practice it is easily carried out. In the directions below, based on an actual series of reaction times, I shall give the details as carried out on a visible model Burroughs, and give the signs S, *, and # just as they are printed by that machine. The

change in procedure for any other machine which operates by pulling a lever or turning a crank is readily made. Of course, if a non-printing machine is used, the totals must be written down.

174	195	178	181	−170
200	185	−158	193	176
181	−144	−164	182	−140
−158	−166	−167	−170	−155
188	179	183	−170	−159
216	−163	−171	−148	−159
180	184	−170	−163	−159
−167	179	176	179	195
192	−144	173	−168	−158
−158	−155	184	−165	−167

List all the items in the set. The item counter having been previously set at zero, it will now show that fifty terms have been listed, and therefore that 50 is the correct divisor. Print the subtotal (8,589.00S) which leaves the total still on the wheels. Set up the total on the keys, directly over the figures on the wheels, and pull the handle once, which multiplies the total by 2. Now print the total (17,178.00*) and point off two more places from the right. This gives the average, which may be printed directly under the total, using the non-add key (171.78#). In cases where the divisor is not 50, 25, or 20, it is necessary to divide out, instead of multiplying and pointing off, which takes a little more time.

Now for the mean variation by the second rule. Tear off the printed slip, and with a pencil check off all the terms less than the average; all the terms, that is, which are 171 or less. These are marked in the table above. With the left hand on this strip, list all these items, and print the total (4,336.00*). Now multiply the average (171.78) by the number of terms added (27) as shown by the item counter; print the subtotal (4,638.06S) and from this number (which is still on the wheels) subtract the last total printed (4,336.00). Print the remainder as a subtotal (302.06S), and divide it by half the total number of terms in the series (in this case 25; therefore multiply by four, and point off two places). The result (12.0824) is the *MV*. This result may be reduced to percentage immediately, by dividing it by 171.78; but in practice I find it more economical

to get the MV's of all the sets first, and then to go over the results and reduce the MV's to percentages.

In the operation as described, every step is printed, and there is therefore small chance of an error. In the last division, for example, in multiplying by four, the multiplicand (302.06) is printed once as a subtotal, and three times immediately below that; the four prints prove that the correct number was multiplied by four.

In following the *first* rule, the sum of the terms greater than M, (ΣP) is totaled (4,253.00*), then the product of M by P (23) is totaled (3,950.94*), then ΣP (4.253.00) is put on the wheels again, and $M \cdot P$ (3,950.94) is subtracted from it. This requires one more operation than in the *second* procedure, unless the P terms are counted separately, a procedure which is inadvisable.

In the operations described, the repeat key should be kept down throughout. This is necessary for the multiplying, and in adding, it simplifies the operation; for with the repeat key down, the key in the hundreds column need be struck but five times in adding the fifty terms for the average.

If the machine used has no item-counter, 1 may be set up in a column into which the total will not extend, and will then keep track of the items, and print them. If, for example, on a nine-place machine, 1 is set up in the second column from the left, and units in the terms added at the left of the decimal point, the total in obtaining the average will be 5,008,589.00. This shows that 50 items have been added to a total of 8,589. Or the items may be listed three places farther to the left, and 1 set up in the extreme right-hand column, in which case the total will be 8,589,000.50. On an eleven-place machine, this procedure is feasible for quite large numbers.

A good calculating machine is a labor-saving device of such consequence that it is economically indispensable to a laboratory where research work involving measurements of any sort is being done. One of our men has worked up on the machine in a couple of weeks reaction times which would have required months of labor without the machine; and the machine work has the added advantage of complete reliability in the results.

PSYCHOLOGY AS THE BEHAVIORIST VIEWS IT

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Psychology as the behaviorist views it is a purely objective experimental branch of natural science. Its theoretical goal is the prediction and control of behavior. Introspection forms no essential part of its methods, nor is the scientific value of its data dependent upon the readiness with which they lend themselves to interpretation in terms of consciousness. The behaviorist, in his efforts to get a unitary scheme of animal response, recognizes no dividing line between man and brute. The behavior of man, with all of its refinement and complexity, forms only a part of the behaviorist's total scheme of investigation.

It has been maintained by its followers generally that psychology is a study of the science of the phenomena of consciousness. It has taken as its problem, on the one hand, the analysis of complex mental states (or processes) into simple elementary constituents, and on the other the construction of complex states when the elementary constituents are given. The world of physical objects (stimuli, including here anything which may excite activity in a receptor), which forms the total phenomena of the natural scientist, is looked upon merely as means to an end. That end is the production of mental states that may be 'inspected' or 'observed.' The psychological object of observation in the case of an emotion, for example, is the mental state itself. The problem in emotion is the determination of the number and kind of elementary constituents present, their loci, intensity, order of appearance, etc. It is agreed that introspection is the method *par excellence* by means of which mental states may be manipulated for purposes of psychology. On this assumption, behavior data (including under this term everything which goes under the name of comparative psychology) have no value *per se*. They possess

significance only in so far as they may throw light upon conscious states.[1] Such data must have at least an analogical or indirect reference to belong to the realm of psychology.

Indeed, at times, one finds psychologists who are sceptical of even this analogical reference. Such scepticism is often shown by the question which is put to the student of behavior, "what is the bearing of animal work upon human psychology?" I used to have to study over this question. Indeed it always embarrassed me somewhat. I was interested in my own work and felt that it was important, and yet I could not trace any close connection between it and psychology as my questioner understood psychology. I hope that such a confession will clear the atmosphere to such an extent that we will no longer have to work under false pretences. We must frankly admit that the facts so important to us which we have been able to glean from extended work upon the senses of animals by the behavior method have contributed only in a fragmentary way to the general theory of human sense organ processes, nor have they suggested new points of experimental attack. The enormous number of experiments which we have carried out upon learning have likewise contributed little to human psychology. It seems reasonably clear that some kind of compromise must be effected: either psychology must change its viewpoint so as to take in facts of behavior, whether or not they have bearings upon the problems of 'consciousness'; or else behavior must stand alone as a wholly separate and independent science. Should human psychologists fail to look with favor upon our overtures and refuse to modify their position, the behaviorists will be driven to using human beings as subjects and to employ methods of investigation which are exactly comparable to those now employed in the animal work.

Any other hypothesis than that which admits the independent value of behavior material, regardless of any bearing such material may have upon consciousness, will inevitably force us to the absurd position of attempting to *construct* the conscious content of the animal whose behavior we have been studying.

[1] That is, either directly upon the conscious state of the observer or indirectly upon the conscious state of the experimenter.

On this view, after having determined our animal's ability to learn, the simplicity or complexity of its methods of learning, the effect of past habit upon present response, the range of stimuli to which it ordinarily responds, the widened range to which it can respond under experimental conditions,—in more general terms, its various problems and its various ways of solving them,—we should still feel that the task is unfinished and that the results are worthless, until we can interpret them by analogy in the light of consciousness. Although we have solved our problem we feel uneasy and unrestful because of our definition of psychology: we feel forced to say something about the possible mental processes of our animal. We say that, having no eyes, its stream of consciousness cannot contain brightness and color sensations as we know them,—having no taste buds this stream can contain no sensations of sweet, sour, salt and bitter. But on the other hand, since it does respond to thermal, tactual and organic stimuli, its conscious content must be made up largely of these sensations; and we usually add, to protect ourselves against the reproach of being anthropomorphic, "if it has any consciousness." Surely this doctrine which calls for an analogical interpretation of all behavior data may be shown to be false: the position that the standing of an observation upon behavior is determined by its fruitfulness in yielding results which are interpretable only in the narrow realm of (really human) consciousness.

This emphasis upon analogy in psychology has led the behaviorist somewhat afield. Not being willing to throw off the yoke of consciousness he feels impelled to make a place in the scheme of behavior where the rise of consciousness can be determined. This point has been a shifting one. A few years ago certain animals were supposed to possess 'associative memory,' while certain others were supposed to lack it. One meets this search for the origin of consciousness under a good many disguises. Some of our texts state that consciousness arises at the moment when reflex and instinctive activities fail properly to conserve the organism. A perfectly adjusted organism would be lacking in consciousness. On the other hand whenever we find the presence of diffuse activity which

results in habit formation, we are justified in assuming consciousness. I must confess that these arguments had weight with me when I began the study of behavior. I fear that a good many of us are still viewing behavior problems with something like this in mind. More than one student in behavior has attempted to frame criteria of the psychic—to devise a set of objective, structural and functional criteria which, when applied in the particular instance, will enable us to decide whether such and such responses are positively conscious, merely indicative of consciousness, or whether they are purely 'physiological.' Such problems as these can no longer satisfy behavior men. It would be better to give up the province altogether and admit frankly that the study of the behavior of animals has no justification, than to admit that our search is of such a 'will o' the wisp' character. One can assume either the presence or the absence of consciousness anywhere in the phylogenetic scale without affecting the problems of behavior by one jot or one tittle; and without influencing in any way the mode of experimental attack upon them. On the other hand, I cannot for one moment assume that the paramecium responds to light; that the rat learns a problem more quickly by working at the task five times a day than once a day, or that the human child exhibits plateaux in his learning curves. These are questions which vitally concern behavior and which must be decided by direct observation under experimental conditions.

This attempt to reason by analogy from human conscious processes to the conscious processes in animals, and *vice versa:* to make consciousness, as the human being knows it, the center of reference of all behavior, forces us into a situation similar to that which existed in biology in Darwin's time. The whole Darwinian movement was judged by the bearing it had upon the origin and development of the human race. Expeditions were undertaken to collect material which would establish the position that the rise of the human race was a perfectly natural phenomenon and not an act of special creation. Variations were carefully sought along with the evidence for the heaping up effect and the weeding out effect of selection; for in these

and the other Darwinian mechanisms were to be found factors sufficiently complex to account for the origin and race differentiation of man. The wealth of material collected at this time was considered valuable largely in so far as it tended to develop the concept of evolution in man. It is strange that this situation should have remained the dominant one in biology for so many years. The moment zoölogy undertook the experimental study of evolution and descent, the situation immediately changed. Man ceased to be the center of reference. I doubt if any experimental biologist today, unless actually engaged in the problem of race differentiation in man, tries to interpret his findings in terms of human evolution, or ever refers to it in his thinking. He gathers his data from the study of many species of plants and animals and tries to work out the laws of inheritance in the particular type upon which he is conducting experiments. Naturally, he follows the progress of the work upon race differentiation in man and in the descent of man, but he looks upon these as special topics, equal in importance with his own yet ones in which his interests will never be vitally engaged. It is not fair to say that all of his work is directed toward human evolution or that it must be interpreted in terms of human evolution. He does not have to dismiss certain of his facts on the inheritance of coat color in mice because, forsooth, they have little bearing upon the differentiation of the *genus homo* into separate races, or upon the descent of the *genus homo* from some more primitive stock.

In psychology we are still in that stage of development where we feel that we must select our material. We have a general place of discard for processes, which we anathematize so far as their value for psychology is concerned by saying, "this is a reflex"; "that is a purely physiological fact which has nothing to do with psychology." We are not interested (as psychologists) in getting all of the processes of adjustment which the animal as a whole employs, and in finding how these various responses are associated, and how they fall apart, thus working out a systematic scheme for the prediction and control of response in general. Unless our observed facts are indicative

of consciousness, we have no use for them, and unless our apparatus and method are designed to throw such facts into relief, they are thought of in just as disparaging a way. I shall always remember the remark one distinguished psychologist made as he looked over the color apparatus designed for testing the responses of animals to monochromatic light in the attic at Johns Hopkins. It was this: "And they call this psychology!"

I do not wish unduly to criticize psychology. It has failed signally, I believe, during the fifty-odd years of its existence as an experimental discipline to make its place in the world as an undisputed natural science. Psychology, as it is generally thought of, has something esoteric in its methods. If you fail to reproduce my findings, it is not due to some fault in your apparatus or in the control of your stimulus, but it is due to the fact that your introspection is untrained.[1] The attack is made upon the observer and not upon the experimental setting. In physics and in chemistry the attack is made upon the experimental conditions. The apparatus was not sensitive enough, impure chemicals were used, etc. In these sciences a better technique will give reproducible results. Psychology is otherwise. If you can't observe 3–9 states of clearness in attention, your introspection is poor. If, on the other hand, a feeling seems reasonably clear to you, your introspection is again faulty. You are seeing too much. Feelings are never clear.

The time seems to have come when psychology must discard all reference to consciousness; when it need no longer delude itself into thinking that it is making mental states the object of observation. We have become so enmeshed in speculative questions concerning the elements of mind, the nature of conscious content (for example, imageless thought, attitudes, and Bewusseinslage, etc.) that I, as an experimental student, feel that something is wrong with our premises and the types of problems which develop from them. There is no longer

[1] In this connection I call attention to the controversy now on between the adherents and the opposers of imageless thought. The 'types of reactors' (sensory and motor) were also matters of bitter dispute. The complication experiment was the source of another war of words concerning the accuracy of the opponents' introspection

any guarantee that we all mean the same thing when we use the terms now current in psychology. Take the case of sensation. A sensation is defined in terms of its attributes. One psychologist will state with readiness that the attributes of a visual sensation are *quality*, *extension*, *duration*, and *intensity*. Another will add *clearness*. Still another that of *order*. I doubt if any one psychologist can draw up a set of statements describing what he means by sensation which will be agreed to by three other psychologists of different training. Turn for a moment to the question of the number of isolable sensations. Is there an extremely large number of color sensations—or only four, red, green, yellow and blue? Again, yellow, while psychologically simple, can be obtained by superimposing red and green spectral rays upon the same diffusing surface! If, on the other hand, we say that every just noticeable difference in the spectrum is a simple sensation, and that every just noticeable increase in the white value of a given color gives simple sensations, we are forced to admit that the number is so large and the conditions for obtaining them so complex that the concept of sensation is unusable, either for the purpose of analysis or that of synthesis. Titchener, who has fought the most valiant fight in this country for a psychology based upon introspection, feels that these differences of opinion as to the number of sensations and their attributes; as to whether there are relations (in the sense of elements) and on the many others which seem to be fundamental in every attempt at analysis, are perfectly natural in the present undeveloped state of psychology. While it is admitted that every growing science is full of unanswered questions, surely only those who are wedded to the system as we now have it, who have fought and suffered for it, can confidently believe that there will ever be any greater uniformity than there is now in the answers we have to such questions. I firmly believe that two hundred years from now, unless the introspective method is discarded, psychology will still be divided on the question as to whether auditory sensations have the quality of 'extension,' whether intensity is an attribute which can be applied to color, whether there is a difference in 'texture' between image and sensation and upon many hundreds of others of like character.

The condition in regard to other mental processes is just as chaotic. Can image type be experimentally tested and verified? Are recondite thought processes dependent mechanically upon imagery at all? Are psychologists agreed upon what feeling is? One states that feelings are attitudes. Another finds them to be groups of organic sensations possessing a certain solidarity. Still another and larger group finds them to be new elements correlative with and ranking equally with sensations.

My psychological quarrel is not with the systematic and structural psychologist alone. The last fifteen years have seen the growth of what is called functional psychology. This type of psychology decries the use of elements in the static sense of the structuralists. It throws emphasis upon the biological significance of conscious processes instead of upon the analysis of conscious states into introspectively isolable elements. I have done my best to understand the difference between functional psychology and structural psychology. Instead of clarity, confusion grows upon me. The terms sensation, perception, affection, emotion, volition are used as much by the functionalist as by the structuralist. The addition of the word 'process' ('mental act as a whole,' and like terms are frequently met) after each serves in some way to remove the corpse of 'content' and to leave 'function' in its stead. Surely if these concepts are elusive when looked at from a content standpoint, they are still more deceptive when viewed from the angle of function, and especially so when function is obtained by the introspection method. It is rather interesting that no functional psychologist has carefully distinguished between 'perception' (and this is true of the other psychological terms as well) as employed by the systematist, and 'perceptual process' as used in functional psychology. It seems illogical and hardly fair to criticize the psychology which the systematist gives us, and then to utilize his terms without carefully showing the changes in meaning which are to be attached to them. I was greatly surprised some time ago when I opened Pillsbury's book and saw psychology defined as the 'science of behavior.' A still more recent text states that

psychology is the 'science of mental behavior.' When I saw these promising statements I thought, now surely we will have texts based upon different lines. After a few pages the science of behavior is dropped and one finds the conventional treatment of sensation, perception, imagery, etc., along with certain shifts in emphasis and additional facts which serve to give the author's personal imprint.

One of the difficulties in the way of a consistent functional psychology is the parallelistic hypothesis. If the functionalist attempts to express his formulations in terms which make mental states really appear to function, to play some active rôle in the world of adjustment, he almost inevitably lapses into terms which are connotative of interaction. When taxed with this he replies that it is more convenient to do so and that he does it to avoid the circumlocution and clumsiness which are inherent in any thoroughgoing parallelism.[1] As a matter of fact I believe the functionalist actually thinks in terms of interaction and resorts to parallelism only when forced to give expression to his views. I feel that *behaviorism* is the only consistent and logical functionalism. In it one avoids both the Scylla of parallelism and the Charybdis of interaction. Those time-honored relics of philosophical speculation need trouble the student of behavior as little as they trouble the student of physics. The consideration of the mind-body problem affects neither the type of problem selected nor the formulation of the solution of that problem. I can state my position here no better than by saying that I should like to bring my students up in the same ignorance of such hypotheses as one finds among the students of other branches of science.

This leads me to the point where I should like to make the argument constructive. I believe we can write a psychology, define it as Pillsbury, and never go back upon our definition: never use the terms consciousness, mental states, mind, content, introspectively verifiable, imagery, and the like. I believe that we can do it in a few years without running into the absurd

[1] My colleague, Professor H. C. Warren, by whose advice this article was offered to the Review, believes that the parallelist can avoid the interaction terminology completely by exercising a little care.

terminology of Beer, Bethe, Von Uexküll, Nuel, and that of the so-called objective schools generally. It can be done in terms of stimulus and response, in terms of habit formation, habit integrations and the like. Furthermore, I believe that it is really worth while to make this attempt now.

The psychology which I should attempt to build up would take as a starting point, first, the observable fact that organisms, man and animal alike, do adjust themselves to their environment by means of hereditary and habit equipments. These adjustments may be very adequate or they may be so inadequate that the organism barely maintains its existence; secondly, that certain stimuli lead the organisms to make the responses. In a system of psychology completely worked out, given the response the stimuli can be predicted; given the stimuli the response can be predicted. Such a set of statements is crass and raw in the extreme, as all such generalizations must be. Yet they are hardly more raw and less realizable than the ones which appear in the psychology texts of the day. I possibly might illustrate my point better by choosing an everyday problem which anyone is likely to meet in the course of his work. Some time ago I was called upon to make a study of certain species of birds. Until I went to Tortugas I had never seen these birds alive. When I reached there I found the animals doing certain things: some of the acts seemed to work peculiarly well in such an environment, while others seemed to be unsuited to their type of life. I first studied the responses of the group as a whole and later those of individuals. In order to understand more thoroughly the relation between what was habit and what was hereditary in these responses, I took the young birds and reared them. In this way I was able to study the order of appearance of hereditary adjustments and their complexity, and later the beginnings of habit formation. My efforts in determining the stimuli which called forth such adjustments were crude indeed. Consequently my attempts to control behavior and to produce responses at will did not meet with much success. Their food and water, sex and other social relations, light and temperature conditions were all beyond control in a field study. I did find

it possible to control their reactions in a measure by using the nest and egg (or young) as stimuli. It is not necessary in this paper to develop further how such a study should be carried out and how work of this kind must be supplemented by carefully controlled laboratory experiments. Had I been called upon to examine the natives of some of the Australian tribes, I should have gone about my task in the same way. I should have found the problem more difficult: the types of responses called forth by physical stimuli would have been more varied, and the number of effective stimuli larger. I should have had to determine the social setting of their lives in a far more careful way. These savages would be more influenced by the responses of each other than was the case with the birds. Furthermore, habits would have been more complex and the influences of past habits upon the present responses would have appeared more clearly. Finally, if I had been called upon to work out the psychology of the educated European, my problem would have required several lifetimes. But in the one I have at my disposal I should have followed the same general line of attack. In the main, my desire in all such work is to gain an accurate knowledge of adjustments and the stimuli calling them forth. My final reason for this is to learn general and particular methods by which I may control behavior. My goal is not "the description and explanation of states of consciousness as such," nor that of obtaining such proficiency in mental gymnastics that I can immediately lay hold of a state of consciousness and say, "this, as a whole, consists of gray sensation number 350, of such and such extent, occurring in conjunction with the sensation of cold of a certain intensity; one of pressure of a certain intensity and extent," and so on *ad infinitum*. If psychology would follow the plan I suggest, the educator, the physician, the jurist and the business man could utilize our data in a practical way, as soon as we are able, experimentally, to obtain them. Those who have occasion to apply psychological principles practically would find no need to complain as they do at the present time. Ask any physician or jurist today whether scientific psychology plays a practical part in his daily routine and you will hear

him deny that the psychology of the laboratories finds a place in his scheme of work. I think the criticism is extremely just. One of the earliest conditions which made me dissatisfied with psychology was the feeling that there was no realm of application for the principles which were being worked out in content terms.

What gives me hope that the behaviorist's position is a defensible one is the fact that those branches of psychology which have already partially withdrawn from the parent, experimental psychology, and which are consequently less dependent upon introspection are today in a most flourishing condition. Experimental pedagogy, the psychology of drugs, the psychology of advertising, legal psychology, the psychology of tests, and psychopathology are all vigorous growths. These are sometimes wrongly called "practical" or "applied" psychology. Surely there was never a worse misnomer. In the future there may grow up vocational bureaus which really apply psychology. At present these fields are truly scientific and are in search of broad generalizations which will lead to the control of human behavior. For example, we find out by experimentation whether a series of stanzas may be acquired more readily if the whole is learned at once, or whether it is more advantageous to learn each stanza separately and then pass to the succeeding. We do not attempt to apply our findings. The application of this principle is purely voluntary on the part of the teacher. In the psychology of drugs we may show the effect upon behavior of certain doses of caffeine. We may reach the conclusion that caffeine has a good effect upon the speed and accuracy of work. But these are general principles. We leave it to the individual as to whether the results of our tests shall be applied or not. Again, in legal testimony, we test the effects of recency upon the reliability of a witness's report. We test the accuracy of the report with respect to moving objects, stationary objects, color, etc. It depends upon the judicial machinery of the country to decide whether these facts are ever to be applied. For a 'pure' psychologist to say that he is not interested in the questions raised in these divisions of the science because they relate

indirectly to the application of psychology shows, in the first place, that he fails to understand the scientific aim in such problems, and secondly, that he is not interested in a psychology which concerns itself with human life. The only fault I have to find with these disciplines is that much of their material is stated in terms of introspection, whereas a statement in terms of objective results would be far more valuable. There is no reason why appeal should ever be made to consciousness in any of them. Or why introspective data should ever be sought during the experimentation, or published in the results. In experimental pedagogy especially one can see the desirability of keeping all of the results on a purely objective plane. If this is done, work there on the human being will be comparable directly with the work upon animals. For example, at Hopkins, Mr. Ulrich has obtained certain results upon the distribution of effort in learning—using rats as subjects. He is prepared to give comparative results upon the effect of having an animal work at the problem once per day, three times per day, and five times per day. Whether it is advisable to have the animal learn only one problem at a time or to learn three abreast. We need to have similar experiments made upon man, but we care as little about his 'conscious processes' during the conduct of the experiment as we care about such processes in the rats.

I am more interested at the present moment in trying to show the necessity for maintaining uniformity in experimental procedure and in the method of stating results in both human and animal work, than in developing any ideas I may have upon the changes which are certain to come in the scope of human psychology. Let us consider for a moment the subject of the range of stimuli to which animals respond. I shall speak first of the work upon vision in animals. We put our animal in a situation where he will respond (or learn to respond) to one of two monochromatic lights. We feed him at the one (positive) and punish him at the other (negative). In a short time the animal learns to go to the light at which he is fed. At this point questions arise which I may phrase in two ways: I may choose the psychological way and say

"does the animal see these two lights as I do, *i. e.*, as two distinct colors, or does he see them as two grays differing in brightness, as does the totally color blind?" Phrased by the behaviorist, it would read as follows: "Is my animal responding upon the basis of the difference in intensity between the two stimuli, or upon the difference in wave-lengths?" He nowhere thinks of the animal's response in terms of his own experiences of colors and grays. He wishes to establish the fact whether wave-length is a factor in that animal's adjustment.[1] If so, what wave-lengths are effective and what differences in wave-length must be maintained in the different regions to afford bases for differential responses? If wave-length is not a factor in adjustment he wishes to know what difference in intensity will serve as a basis for response, and whether that same difference will suffice throughout the spectrum. Furthermore, he wishes to test whether the animal can respond to wave-lengths which do not affect the human eye. He is as much interested in comparing the rat's spectrum with that of the chick as in comparing it with man's. The point of view when the various sets of comparisons are made does not change in the slightest.

However we phrase the question to ourselves, we take our animal after the association has been formed and then introduce certain control experiments which enable us to return answers to the questions just raised. But there is just as keen a desire on our part to test man under the same conditions, and to state the results in both cases in common terms.

The man and the animal should be placed as nearly as possible under the same experimental conditions. Instead of feeding or punishing the human subject, we should ask him to respond by setting a second apparatus until standard and control offered no basis for a differential response. Do I lay myself open to the charge here that I am using introspection? My reply is not at all; that while I might very well feed my human subject for a right choice and punish him for a wrong one and thus produce the response if the subject could give it,

[1] He would have exactly the same attitude as if he were conducting an experiment to show whether an ant would crawl over a pencil laid across the trail or go round it.

there is no need of going to extremes even on the platform I suggest. But be it understood that I am merely using this second method as an abridged behavior method.[1] We can go just as far and reach just as dependable results by the longer method as by the abridged. In many cases the direct and typically human method cannot be safely used. Suppose, for example, that I doubt the accuracy of the setting of the control instrument, in the above experiment, as I am very likely to do if I suspect a defect in vision? It is hopeless for me to get his introspective report. He will say: "There is no difference in sensation, both are reds, identical in quality." But suppose I confront him with the standard and the control and so arrange conditions that he is punished if he responds to the 'control' but not with the standard. I interchange the positions of the standard and the control at will and force him to attempt to differentiate the one from the other. If he can learn to make the adjustment even after a large number of trials it is evident that the two stimuli do afford the basis for a differential response. Such a method may sound nonsensical, but I firmly believe we will have to resort increasingly to just such method where we have reason to distrust the language method.

There is hardly a problem in human vision which is not also a problem in animal vision: I mention the limits of the spectrum, threshold values, absolute and relative, flicker, Talbot's law, Weber's law, field of vision, the Purkinje phenomenon, etc. Every one is capable of being worked out by behavior methods. Many of them are being worked out at the present time.

I feel that all the work upon the senses can be consistently

[1] I should prefer to look upon this abbreviated method, where the human subject is told in words, for example, to equate two stimuli; or to state in words whether a given stimulus is present or absent, etc., as the *language method* in behavior. It in no way changes the status of experimentation. The method becomes possible merely by virtue of the fact that in the particular case the experimenter and his animal have systems of abbreviations or shorthand behavior signs (language), any one of which may stand for a habit belonging to the repertoire both of the experimenter and his subject. To make the data obtained by the language method virtually the whole of behavior—or to attempt to mould all of the data obtained by other methods in terms of the one which has by all odds the most limited range—is putting the cart before the horse with a vengeance.

carried forward along the lines I have suggested here for vision. Our results will, in the end, give an excellent picture of what each organ stands for in the way of function. The anatomist and the physiologist may take our data and show, on the one hand, the structures which are responsible for these responses, and, on the other, the physico-chemical relations which are necessarily involved (physiological chemistry of nerve and muscle) in these and other reactions.

The situation in regard to the study of memory is hardly different. Nearly all of the memory methods in actual use in the laboratory today yield the type of results I am arguing for. A certain series of nonsense syllables or other material is presented to the human subject. What should receive the emphasis are the rapidity of the habit formation, the errors, peculiarities in the form of the curve, the persistence of the habit so formed, the relation of such habits to those formed when more complex material is used, etc. Now such results are taken down with the subject's introspection. The experiments are made for the purpose of discussing the mental machinery[1] involved in learning, in recall, recollection and forgetting, and not for the purpose of seeking the human being's way of shaping his responses to meet the problems in the terribly complex environment into which he is thrown, nor for that of showing the similarities and differences between man's methods and those of other animals.

The situation is somewhat different when we come to a study of the more complex forms of behavior, such as imagination, judgment, reasoning, and conception. At present the only statements we have of them are in content terms.[2]

[1] They are often undertaken apparently for the purpose of making crude pictures of what must or must not go on in the nervous system.

[2] There is need of questioning more and more the existence of what psychology calls imagery. Until a few years ago I thought that centrally aroused visual sensations were as clear as those peripherally aroused. I had never accredited myself with any other kind. However, closer examination leads me to deny in my own case the presence of imagery in the Galtonian sense. The whole doctrine of the centrally aroused image is, I believe, at present, on a very insecure foundation. Angell as well as Fernald reach the conclusion that an objective determination of image type is impossible. It would be an interesting confirmation of their experimental work if we should find by degrees that we have been mistaken in building up this enormous structure of the centrally aroused sensation (or image).

Our minds have been so warped by the fifty-odd years which have been devoted to the study of states of consciousness that we can envisage these problems only in one way. We should meet the situation squarely and say that we are not able

The hypothesis that all of the so-called 'higher thought' processes go on in terms of faint reinstatements of the original muscular act (including speech here) and that these are integrated into systems which respond in serial order (associative mechanisms) is, I believe, a tenable one. It makes reflective processes as mechanical as habit. The scheme of habit which James long ago described—where each return or afferent current releases the next appropriate motor discharge—is as true for 'thought processes' as for overt muscular acts. Paucity of 'imagery' would be the rule. In other words, wherever there are thought processes there are faint contractions of the systems of musculature involved in the overt exercise of the customary act, and especially in the still finer systems of musculature involved in speech. If this is true, and I do not see how it can be gainsaid, imagery becomes a mental luxury (even if it really exists) without any functional significance whatever. If experimental procedure justifies this hypothesis, we shall have at hand tangible phenomena which may be studied as behavior material. I should say that the day when we can study reflective processes by such methods is about as far off as the day when we can tell by physico-chemical methods the difference in the structure and arrangement of molecules between living protoplasm and inorganic substances. The solutions of both problems await the advent of methods and apparatus.

[After writing this paper I heard the addresses of Professors Thorndike and Angell, at the Cleveland meeting of the American Psychological Association. I hope to have the opportunity to discuss them at another time. I must even here attempt to answer one question raised by Thorndike.

Thorndike (see this issue) casts suspicions upon ideo-motor action. If by ideo-motor action he means just that and would not include sensori-motor action in his general denunciation, I heartily agree with him. I should throw out imagery altogether and attempt to show that practically all natural thought goes on in terms of sensori-motor processes in the larynx (but not in terms of 'imageless thought') which rarely come to consciousness in any person who has not groped for imagery in the psychological laboratory. This easily explains why so many of the well-educated laity know nothing of imagery. I doubt if Thorndike conceives of the matter in this way. He and Woodworth seem to have neglected the speech mechanisms.

It has been shown that improvement in habit comes unconsciously. The first we know of it is when it is achieved—when it becomes an object. I believe that 'consciousness' has just as little to do with *improvement* in thought processes. Since, according to my view, thought processes are really motor habits in the larynx, improvements, short cuts, changes, etc., in these habits are brought about in the same way that such changes are produced in other motor habits. This view carries with it the implication that there are no reflective processes (centrally initiated processes): The individual is always *examining objects*, in the one case objects in the now accepted sense, in the other their substitutes, viz., the movements in the speech musculature. From this it follows that there is no theoretical limitation of the behavior method. There remains, to be sure, the practical difficulty, which may never be overcome, of examining speech movements in the way that general bodily behavior may be examined.]

to carry forward investigations along all of these lines by the behavior methods which are in use at the present time. In extenuation I should like to call attention to the paragraph above where I made the point that the introspective method itself has reached a *cul-de-sac* with respect to them. The topics have become so threadbare from much handling that they may well be put away for a time. As our methods become better developed it will be possible to undertake investigations of more and more complex forms of behavior. Problems which are now laid aside will again become imperative, but they can be viewed as they arise from a new angle and in more concrete settings.

Will there be left over in psychology a world of pure psychics, to use Yerkes' term? I confess I do not know. The plans which I most favor for psychology lead practically to the ignoring of consciousness in the sense that that term is used by psychologists today. I have virtually denied that this realm of psychics is open to experimental investigation. I don't wish to go further into the problem at present because it leads inevitably over into metaphysics. If you will grant the behaviorist the right to use consciousness in the same way that other natural scientists employ it—that is, without making consciousness a special object of observation—you have granted all that my thesis requires.

In concluding, I suppose I must confess to a deep bias on these questions. I have devoted nearly twelve years to experimentation on animals. It is natural that such a one should drift into a theoretical position which is in harmony with his experimental work. Possibly I have put up a straw man and have been fighting that. There may be no absolute lack of harmony between the position outlined here and that of functional psychology. I am inclined to think, however, that the two positions cannot be easily harmonized. Certainly the position I advocate is weak enough at present and can be attacked from many standpoints. Yet when all this is admitted I still feel that the considerations which I have urged should have a wide influence upon the type of psychology which is to be developed in the future. What we need to do

is to start work upon psychology, making *behavior*, not *consciousness*, the objective point of our attack. Certainly there are enough problems in the control of behavior to keep us all working many lifetimes without ever allowing us time to think of consciousness *an sich*. Once launched in the undertaking, we will find ourselves in a short time as far divorced from an introspective psychology as the psychology of the present time is divorced from faculty psychology.

Summary

1. Human psychology has failed to make good its claim as a natural science. Due to a mistaken notion that its fields of facts are conscious phenomena and that introspection is the only direct method of ascertaining these facts, it has enmeshed itself in a series of speculative questions which, while fundamental to its present tenets, are not open to experimental treatment. In the pursuit of answers to these questions, it has become further and further divorced from contact with problems which vitally concern human interest.

2. Psychology, as the behaviorist views it, is a purely objective, experimental branch of natural science which needs introspection as little as do the sciences of chemistry and physics. It is granted that the behavior of animals can be investigated without appeal to consciousness. Heretofore the viewpoint has been that such data have value only in so far as they can be interpreted by analogy in terms of consciousness. The position is taken here that the behavior of man and the behavior of animals must be considered on the same plane; as being equally essential to a general understanding of behavior. It can dispense with consciousness in a psychological sense. The separate observation of 'states of consciousness' is, on this assumption, no more a part of the task of the psychologist than of the physicist. We might call this the return to a non-reflective and naïve use of consciousness. In this sense consciousness may be said to be the instrument or tool with which all scientists work. Whether or not the tool is properly used at present by scientists is a problem for philosophy and not for psychology.

3. From the viewpoint here suggested the facts on the behavior of amœbæ have value in and for themselves without reference to the behavior of man. In biology studies on race differentiation and inheritance in amœbæ form a separate division of study which must be evaluated in terms of the laws found there. The conclusions so reached may not hold in any other form. Regardless of the possible lack of generality, such studies must be made if evolution as a whole is ever to be regulated and controlled. Similarly the laws of behavior in amœbæ, the range of responses, and the determination of effective stimuli, of habit formation, persistency of habits, interference and reinforcement of habits, must be determined and evaluated in and for themselves, regardless of their generality, or of their bearing upon such laws in other forms, if the phenomena of behavior are ever to be brought within the sphere of scientific control.

4. This suggested elimination of states of consciousness as proper objects of investigation in themselves will remove the barrier from psychology which exists between it and the other sciences. The findings of psychology become the functional correlates of structure and lend themselves to explanation in physico-chemical terms.

5. Psychology as behavior will, after all, have to neglect but few of the really essential problems with which psychology as an introspective science now concerns itself. In all probability even this residue of problems may be phrased in such a way that refined methods in behavior (which certainly must come) will lead to their solution.

A PROTEST

To the Editor of the Psychological Review,

Dear Sir:

I beg leave to a protest mildly against Mr. Dunlap's attempt in the November number of this Review to sort me with the Scholastics. I have a great regard for the Scholastics and I much doubt whether any philosophical movement since their day has enlisted more acute thinkers. All the same I do not recognize in Mr. Dunlap's description of them any doctrines closely resembling my own. I dislike to suppose that Mr. Dunlap has read no further than the phrase which he quotes from my Psychology. If he has, he certainly has managed to interpret me in ways I never dreamed of. Perhaps I can in no way heap more coals of fire upon his brow than by saying that I have read his interesting article with the liveliest satisfaction and the constructive portions of it with very extended agreement.

I am, Sir,

Repectfully yours,

James R. Angell

University of Chicago,
December 28, 1912

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